VOL. XVIII. No. 2. March, 1911.

THE PSYCHOLOGICAL REVIEW

THE PLACE OF MOVEMENT IN CONSCIOUSNESS[1]

BY W. B. PILLSBURY

All would agree, I think, that the most important advance in psychological theory in recent years is the enhanced value placed upon movement in explaining mental processes. This development has been most marked in America. Here the explanation in terms of movement has been carried out more systematically and to greater extremes. James initiated the movement with his theory of emotion; it was continued and amplified by Dewey and in one form and another has been extended by many of the most vigorous writers among the younger psychologists, not to mention the impetus it undoubtedly gave to the pragmatic movement in philosophy. The new theory has introduced more of practical interest into psychology, has made a break in the development of the theories of consciousness as pure thought, and has increased the tendency toward the concrete and away from the abstract. More recently the motor theory has gone very far. A reader of some of the texts lately published would be inclined to believe that there was nothing in consciousness but movement, and that the presence of sense organs, or of sensory and associatory tracts in the cortex was at the least a mistake on the part of the Creator. All that seems really necessary is the motor cortex with its muscular connections and an occasional path from the sense organs. The marked disparity between these extremes of the theory and the earlier and popular present belief, together

[1] Address of the president, before the American Psychological Association, Minneapolis Meeting, December, 1910.

with the fact that the theory has been little criticized, suggest that it might be profitable to attempt a critical if sympathetic survey of the different formulations of the theory and to compare it with the facts. The theory is so simple and so easy to present that every one is glad to believe it. The only question that any one cares to raise is how much of it will the known facts permit one to accept.

The motor processes have been used by different men and at different times in the explanation of practically every one of the psychological problems. Berkeley gave a motor interpretation to space and has been followed by some writer in every generation. Time, rhythm, and each of the perception qualities has been referred to movement; Bain found movements important in abstract ideas, and the more recent theories have added practically every one of the cognitive processes to the list. If we add to these the affective processes, the clearness of the attentive consciousness, and the subordinate motor processes, there is nothing in mind that has not been explained in terms of movement. The history of the motor theory or even a complete catalogue of the phenomena that have been explained by it would take too long and is doubtless unnecessary in this connection. For purposes of discussion we may divide the explanations that are made through movement or motor processes into three groups,—the explanations of structures, of functions, and of the higher intellectual processes that lie on the border-line between structure and function.

Of the mental qualities the non-sensory components of perceptions and the memory processes have been most frequently ascribed to movement. Space nearly always involves movement for its full appreciation, and it is simplest to say at once that the movements, real or implied, are space or make spatial appreciation possible. Time and rhythm also imply movement of some sort and find ready explanation as motor processes. Memories can be referred to movement by the same sort of reasoning. It has long been recognized that all memories might be in terms of related kinæsthetic imagery. Stricker suggested that kinæsthetic imagery really

was due to revived movements and that it might be merely a reinstatement of old movements. It would follow, then, that all memory was nothing more or less than habit, and that all memory content was neither more nor less than appreciation of present movements revived on some appropriate occasion. No one, so far as I know, has taken the next logical step and suggested that the true quality of sensation is motor. But it is not impossible. All that is necessary is to assume that each sense organ is connected with definite muscles, that these muscles are all excited whenever the sense organ is stimulated and that the colors or tones that we see or hear have their origin in some group of muscles rather than in a sense organ or in the cortex. If we add this extension, all the qualities of consciousness have their origin in the muscles; the brain is merely a switching station through which the real seats of consciousness may be aroused when the appropriate stimulus presents itself.

The most obvious question that arises in this connection is whether the number of possible movements is sufficient to provide the qualities assumed in the theory, and whether the movements are sufficiently accurate and easily enough discriminated to supply all of the qualities. Stumpf long ago pointed out that the area of greatest sensitivity on the retina was within the region for which no movements of adjustment are necessary and the work of Judd and McAllister seems to reinforce this opinion by showing that one is constantly making unappreciated eye-movements that are greater than the smallest space that may be discriminated. It is very difficult to assume that one may make a different sort of movement for everything that can be remembered. The number of movements seems too few and it is too difficult to appreciate the differences between movements. Then again nothing seems to be gained by taking sensations in through the sense organs and out through the muscles, particularly if we accept the current belief that movements themselves are known only through sensations. One has gone a long way round to get sensations again, and when the result is obtained all the problems are still before us. The

circumlocution gives no real gain. We trade colors, sounds, smells and what not for kinæsthetic sensations that are in every respect as hard to deal with as the original.

All this argument is on the assumption that movements are appreciated only through the sensations that come to the cortex from the moving muscles. That was the conclusion that was reached fifteen years ago. Münsterberg, James, Ferrier and others succeeded in convincing the world of science that the old accepted innervation sense had no existence in fact. Even so doughty a scientific warrior as Wundt gave up his position and tacitly accepted his opponent's views. Seldom has a scientific judgment been so quickly and completely reversed in so short a time. But the advocates of the motor theory seem now to attempt to revamp the innervation sense and reinstate it in its essentials, and that without any new proof. Thus we find Münsterberg arguing that the quality of clearness that arises in attention is due to the action of motor cells. A conscious quality is ascribed to the motor area. In several of the other theories we find a statement or implication that the qualities that are ascribed to consciousness as a result of movement have their origin in the cortical motor apparatus rather than in the sensory ends in the body of the muscles.

This change in the point of view is the more surprising since, so far as I have been able to discover, no new facts have been brought to light that favor the view, and the theories themselves attempt no new interpretation of the old facts. The statements are justified only from theoretical needs. Stated in the current form, the theory is obviously very difficult to disprove and is even harder to prove from any definite evidence. The motor centers are not open to observation and cannot be stimulated in any way that would leave the sensory and associatory areas unaffected. It is thus difficult to prove in the ordinary way that the conscious experiences ascribed to the motor areas do or do not arise from them rather than from the other cortical regions or from the more peripheral motor apparatus. Only two crucial experiments suggest themselves. One would be to examine

a case of brain lesion in which the motor areas alone were affected and determine if the qualities in question were still retained. The fatal defect in this test is that if all the motor regions were destroyed there would be no way of communicating with the patient to know what the character of his consciousness might be. The second would be to stimulate the motor regions of a conscious subject and determine what mental states were produced. This test has recently been tried with another object in view. You are doubtless all familiar with the recent experiments of Cushing. You will recall that he performed the plastic flap operation upon two patients under primary anæsthesia and then permitted them to return to consciousness with the central regions of the cortex exposed. These regions were then stimulated electrically and the patients requested to describe their experiences. The localization of the different motor functions was confirmed. The muscles moved easily with the excitation of the appropriate areas. Here is an experiment that to all appearances is strictly adapted to our problem. The motor regions of the cortex are stimulated vigorously with no excitation of sensory or associatory region. On the theories in question one would expect the richest sort of conscious experience. There should have been wonderful appreciation of space relations. All the ideas should have become unusually clear and distinct, and there should have been a rush of recognitory experiences and of feelings of identity. No conscious process would have been entirely unexpected. For this theory the results were completely disappointing. All that either patient felt was a contraction in the muscle, a contraction that was described as exactly like the one produced when the electric stimulus was applied to the muscle itself. And the patients were expert in detecting such movements since part of the earlier treatment had consisted in exciting contractions in these muscles by the induction current. The results are exactly what the strongest opponent of the innervation theory would expect.

The only escape from the conclusion for the exponents of the theories that ascribe conscious qualities to the motor

areas is to argue that the method of stimulation did not call out the same sort of response that would be called out by the ordinary stimulation through sensory and association areas. While one must admit that this argument has some force, to accept it removes the motor theory from all possibility of direct proof. It would then say in effect that the only sort of excitation of the motor areas that gives rise to consciousness is the one that originates in the sensory and associatory regions, and that at once prepares the way for the answer that it is these other areas of the cortex that supply the conscious qualities. All experimental results, then, since the case for the motor regions as the seat of consciousness was reopened, and these most striking neurological experiments of Cushing confirm the judgment that was passed on the case twenty years ago. The evidence all favors the statement that movements are known only through the excitation of the sense ends in the muscles themselves and that these arouse consciousness only as they excite the kinæsthetic areas in the cortex. The large mass of theory that has developed in recent years on the assumption that the motor regions contribute to consciousness flies directly in the face of the evidence. So far as it is possible to know the facts the only structural contributions of movement to consciousness are the kinæsthetic sensations. The attempt to take the motor contributions of consciousness back into the motor cortex and away from the muscles has no warrant in fact. Its only advantage is that it puts the theory in a form that is difficult to disprove, even if it is impossible to prove. Granted that movements are known only by the kinæsthetic sensations, to translate all conscious qualities into motor terms merely transforms all other sorts of sensation into the one sense, and that a sense relatively poor in qualities.

To assert that all memories and all the elements of perception that do not arise immediately from sense are motor in character is to assert only that these are kinæsthetic sensations. And this does not help in the solution of the problem nor does it square with direct observation. To reinstate all sensory experience in motor terms merely reduces all

memories to one sense. To be sure an occasional man does translate all experiences into kinæsthetic terms for the purpose of recall,—is of the kinæsthetic memory type, but he cannot be made a law for all. Direct experiment or observation shows that most men have other qualities in memory and also as supplements of the raw sense materials in perception. Were it not so it would seem a great waste of cortex. There is no reason why the relatively small kinæsthetic area should be the only one used in recall. The only justification for the assertion that motor processes are alone valuable for memory and perception is that both memory and perception deal primarily with meanings rather than with content, but as we shall see meanings are not necessarily motor.

The first of the more dynamic effects ascribed to movement is the selection of one from the possible mental states that might present themselves at any moment. This selection may be of sensations, of ideas or of movements. The fact and its conditions are the same in any case, and the motor explanation has been or may be applied in one instance as well as in another. When ascribed to sensation it receives the name of attention, when applied to ideas it is reason, when applied to the selection of acts it is approximately identical with will. In any one of these cases not the intensity of the original sensation, nor the strength of the particular association or habit determines the character or course of the conscious process, but it is determined by something outside of the immediate experience. Certain representatives of the motor theory insist that the selecting agency in all of these experiences is the state of preparedness of the motor areas that are connected with the receiving sensory region. If two objects present themselves to the sense organs at the same time it is asserted that that one will actually be perceived which finds the sensory apparatus connected with a motor mechanism already active or in a state of preparedness for action. Similarly if an idea is equally closely connected with two other ideas, the one connected with a motor mechanism, active or partly active at the moment, would be recalled. Logically, when a stimulus had become

the initial member in two habits of about equal strength, the choice between the habitual responses would depend upon which set of muscles was at the moment more prepared to act. Selection, on this theory, depends upon the openness of the way out, not upon the force exerted by the stimuli that are coming in.

The difficulties for this explanation become obvious the moment we ask why the motor tracts are in different degrees of preparedness. If one stops with the theory as stated, all selection and, hence, all consciousness is a matter of chance. If the explanation be pushed back a step farther, the only explanation for the momentary openness of one path of motor discharge must be found in some other sensory stimulus, or in sensory stimuli that have been working just before and have left the motor tract in readiness for response. Thus, if I stand with glass in my hand, hesitant whether to drink or to replace the glass upon the desk, it might be said that the decision was in terms of the readiness of one or the other group of muscles. But this tells nothing of the real occasion for the decision or of its important antecedents. That can be answered only if one can say why the one set of motor organs is active, rather than the other. To answer this leads at once back to sensory excitations, old habits, and present circumstances which suggest the old habits. If the throat is giving strong sensations of thirst, the drinking response will be more active; if embarrassment is felt and the presence of the audience suddenly becomes oppressively prominent, the downward response will be the readier. In any case, one cannot understand the situation or explain choice, if one stops with the statement that the motor tracts are active in one way rather than in another. Selection in attention offers the same difficulties, and the motor explanation has the same limitations. Selection, if adequate, must depend upon the knowledge of the environment,—upon the attitude, sensory as well as motor. It may be said that the considerations act first upon the motor tracts and then back upon the sensory and associatory regions, but this is not at all assured. It is altogether probable that the secondary, or preceding sensory determinants, are coör-

dinated in association tracts, rather than first finding their expression in the motor regions. Certainly no one can be sure that the point of interaction and coördination is not in the associatory areas, rather than in the motor regions. In short, the explanation of selection, in all of the forms that the motor theory presents, either is not motor or it is not an explanation. Selection seems to be no more exclusively dependent upon the motor parts of the cortex than are the qualities of consciousness. The important elements in each are the sensory stimuli and their interaction with the results of earlier experiences; the motor areas are altogether subordinate if they play any part at all.

The more general phases of the motor theory and the explanations of the higher mental functions seem to have grown out of very much the same set of considerations. The great advance that has been made in discussing psychology in recent years has consisted in attempting to ascertain the value of the different parts of consciousness. Consciousness as the sum total of sensations or of experiences is a complete chaos. There is no reason why one part should be more important than any other. No one element should stand out prominently over the others. The actual consciousness on the contrary is always unified, is directed to certain ends and organized with the emphasis upon definite parts. The motor theory looks to action at every point as the end and the means of the organization. Action is the usual end of all thinking, just as movement is the ordinary outcome of all action of the nervous system. Not only that, but most consciousness deals not with qualities or particulars of any sort but with events, with groups of qualities, and these groupings, the motor theory asserts, are made by the actions. A thing is not primarily an agglomeration of sensations; it is an occasion for action, and the sort of action goes far to determine the character of the thing. One does not reinstate a group of qualities in remembering an event, one remembers that the event happened. And the thing that happened takes its importance and its character from the actions that it called out or that it might have called out. A memory, then, is

nothing more than some way of representing these earlier actions, and what more suitable than a reinstatement of some element of the old movement. Any form of thought may be referred to movement in the same way. In short, consciousness is like a funnel. The end that receives impressions is very large while the end that lets their effects out into the world through movement is relatively small. The suggestion of the motor theorist is that it is easier to understand the small end than the large; that acts are the natural systematizers of the original chaos of consciousness, and that we should look to them for the organized materials.

All of this must I think be accepted as a general principle. Action is the natural end of thought. Thinking, too, is organized, not chaotic, and organized about acts. Most of thought is only schematically representative of the events and things; the events and things are not represented in detail. The only question is whether action is the only element in the organizing process, and whether the representatives of all things not explicitly reproduced in consciousness must necessarily be motor in character. In general it seems that the motor theorist has been beguiled by the ease of explanation in very much the same way as the older sensationalist. If all sensory processes do lead over into movement and the sensations themselves are too rich in multiplicity and too little organized to explain the well ordered course of thinking, nothing is easier than to say that thought is movement and that all the higher functions, all the representative processes can be explained by the movements that are evoked. This is perfectly simple, and near enough the fact to be plausible. The difficulties begin to present themselves, however, when one asks how the movements can have representative value when taken out of the setting in which they originally presented themselves. How, for example, can the movement that I made when I first thought of a triangle have a greater representative value than an image of a triangle. How in general can any movement be any more representative of anything beside itself than any other particular mental process. It is just a bare movement appre-

ciated through kinæsthetic imagery, as a visual image is a bare image. Neither should have any representative value as it stands. To understand how one can take on meaning is just as difficult as to understand how the other can represent things beyond itself. Certainly a movement has no representative value when it is made for the first time. What changes when the movement does take on meaning is not the movement itself but something added to the movement, and this something might be added to any sensation just as well as to the movement.

The motor theory breaks down in the same way when at all critically examined in its applications to the other functional problems, as well as to meaning. Take the related but slightly simpler problem of recognition. On the motor theory I am supposed to recognize a friend because I make the same movements now that I made when I last saw the same friend. I presume this means that I must have a specifically different smile for each friend, or a characteristic form of salute. Granted that this was the case, although it would require a very large repertoire of smiles, the problem would at once arise how I recognize these smiles as different. That is just as much a problem when translated into movement as when stated in visual terms. This becomes particularly evident when one considers how little our different movements are discriminated in every-day life. One makes movements every moment or two that pass entirely unnoticed. I have corrected my balance hundreds of times during the present half hour without once being conscious of it. The different movements are not at all easily discriminated, as is apparent to any one learning a game of skill. In golf, one thinks the same movement has been made as when one got a good drive, at the last tee, but the ball goes far out of course or does not move. When appreciated, movements are not appreciated for themselves but for their meanings. In this they are like any other sensation. The only way that they are different in this respect from other sensory qualities is that they seem to have rather less distinctive character, to be more completely dependent upon translation into other terms and upon their

connections for their comprehension and interpretation, — for recognition and for the discrimination of one movement from another. Far from being independently appreciated, and a suitable means for the discrimination and reference of other mental qualities, movements are, if possible, more dependent upon outside factors for their appreciation than visual or auditory qualities. They have no meaning of their own as independent entities; they are not easily recognized; they cannot always be identified. They possess none of the almost magical virtues for themselves that they are assumed in the motor theory to be able to impart to the other sensory qualities and mental states.

To understand how any thing or process is recognized or why any image has meaning, one must look not to structure of any sort, but to function, not to any element of consciousness but to the connections of elements in consciousness. The assumption of certain of the motor theorists that motor explanation of the extreme sort and functionalism are identical seems far from self-evident. To be sure there are many things that we think without explicit mental imagery, and the imagery that we have when we think them is indifferent to the things that are thought. This function of the image or of mental representation in general offers one of the most important problems for psychology. It is the problem of use in the broad sense as opposed to the problem of elements or structures. The structuralist has occasionally tried to solve it by transforming function into some peculiar structure different from the ordinarily accepted elements of consciousness. This it must be admitted does not solve or even face the problem in its essence. But the solution of the motor men is on exactly the same level. They would transform function into movement as the sensationalists have at times attempted to transform it into sensation or related content. As is evident from the earlier discussion, movement is as much a structure as sensation, and is as little capable of solving problems of function. The motor theory of itself is not a functional theory, — it is merely a new way of palming off structure as function.

The only other ground on which a motor theory could be called functional or exclusively functional is on the assumption that movement is the end of life and that all of man's accomplishments are of a physical character. On that assumption man's only function is to move, the only outcome of thought is action. But this theory is too superficial to be intended by the motor man. To say that all functions are to be explained by movements would be meaningless if function meant nothing more than movement. All agree that, however important action may be in the final outcome, action is only possible or at least adequate because of a large amount of antecedent thought. Furthermore a large part of the conscious life is important for itself; the result in action is long delayed if present at all, and what seems important at the moment is the intellectual satisfaction, or some other contemplative passive accomplishment. A functional psychology in the larger sense must recognize functions of this sort as well as the products of immediate action. A complete functional psychology must treat mental accomplishments of any sort so far as they are not definitely embodied in structures, or so far as the structures are not prominent nor essential to the accomplishment. Functional problems in whole or in part concern the nature and conditions of selection, the nature of meaning and belief, the reasoning processes in general and much of memory and imagination. It must be admitted with the motor men that movement is nearly always involved in these operations either as antecedent or consequent or both; the only point in controversy is whether the movement is the essential condition of the functions or whether the functions are the natural conditions of the movement. As has been shown above, when carefully examined and tested the explanation in terms of movement does not put us any farther ahead. It is necessary to turn back to examine the antecedents of the movement before one can understand the movement. This is granted by many of the men who uphold the theory that I have been criticizing. They insist however that to ask how the movement originates or to question how it gives value to the con-

scious processes that develop from it clouds the issue. I must confess that I cannot follow them. To admit that a movement is determined by the attitude or the organization, and then to deny that these antecedents have any real value in the explanation seems inconsistent or at the best arbitrary.

When reduced to the last analysis, the difference between the motor theory of these higher processes or functions, and what might be regarded as the true functional theory is really slight. The former would insist that all of the essentials in the explanation are to be found in the movements that are made at the time, while the other finds an explanation in the organization and interconnection of experiences that precede movement. There is no time to go into the nature of this organization now, and complete exposition is the more unnecessary as there is fairly close agreement as to the more fundamental principles. In short there is difference of opinion only as to whether organization or attitude is the result of movement, or whether movement of any rational, intelligent sort is the outcome of organization. The difference is the less important since all would agree that among the important elements in the development of any organization or attitude are the earlier movements that have been made in similar connections, and that movement contributes many important qualities to the consciousness of the functions that do not lead immediately to movement. One believes that movement is the only cause of the organization, the other that it is one important cause of that organization.

One other more general consideration has been adduced in favor of making movement the determining element in mind. It is argued from the fact that movement is the end of all thought, that the movement must induce the thought from which it originated. When for example a period of reflection culminates in a stroke at billiards, the stroke must be regarded as the cause of the thought. One must grant that the thinking would not have gone on as it did if the movement had not been contemplated, but it is a matter of temperament whether one regards the movement as the outcome of

the desire to win and the other antecedent thoughts and habits or of the thoughts as looking forward to the act and that to the outcome of the game. Any fact, mental or physical, can be explained in terms of all the antecedent events in the relevant series or in terms of all the succeeding events in the series, but one gets a different explanation as one looks forward or back for it. One can only insist that if one explains the mental process by the stroke that it starts, one should also be prepared to hold that the movement of the ball induces the stroke of the cue, and that, in turn, the muscular contractions, just as the muscular contractions induce the mental states. There is no hope of settling the rival claims of teleology and causation; all that can be done is to indicate that the explanation under discussion is teleological, not causal.

In this criticism of the motor theory I have refrained for the most part from ascribing the phases of the theory to different authors. This was partly from desire to avoid personal controversy and partly because it was frequently difficult to pin a theory to any one man. Several times when confirming a reference to an author who had been credited with a theory I found that his writings gave little warrant for the view assigned to him. I am quite free to admit that in several instances I may have been fighting wind-mills, but at the risk of belittling my audience I have tried to make clear what wind-mills I was fighting. I believe that each of the positions that I have contested is held to-day by some reputable psychologist, and I presume that there is no psychologist of standing who does not somewhere hold some one of them.

It is not the intention of this address to undervalue the importance of the motor theory or even to refute it in its essentials. As was said in the beginning, it is one of the most vital movements in modern psychology. It does seem, however, that just at present it is being carried beyond the point where it can be justified by the facts, and much as one may regret the necessity it is well to recognize the limits beyond which it cannot be extended. If we summarize, the motor theory is right in emphasizing the part played by kinæsthetic

qualities in every domain. They constitute an important part of every perception process; they color memory, and give tone to all of the intellectual operations. On the active side they constitute the goal of all thought, they have played an enormously important part in the organization of experiences into systems of knowledge, and so in making thought possible. Perhaps more important than all, the motor theory has insisted that no mental process can be understood in and of itself. It has rendered impossible the older form of explanation in which mental states were regarded as entities that were independent of all other mental activities present as well as past. Interrelation, context, attitude are now made the basis of all forms of mental explanation rather than elements or entities. The advance has been a noteworthy one.

On the other hand the motor theory certainly claims too much. Not all qualities of perception or memory are of motor origin. And what is of motor origin cannot be ascribed to the motor cortex directly, but only indirectly through the kinæsthetic sensations. Also no meaning can be given to the assertion that selection of conscious processes is in terms of movement. Rather one must look to the results of organized early experience for an explanation at once of the direction of mental states and of the movements themselves. The attitude of the moment determines movement, not movement the character of the attitude. Again the higher mental functions, although they involve movement, cannot be explained in terms of movement alone. Movements in and of themselves have no meaning, are not immediately recognized nor understood. These functions require just as much explanation when they attach to movements as to any mental process. Movements, too, acquire meaning, are recognized or judged only as they may be referred to other mental states and finally to a systematized knowledge. To identify the motor theory with a functional psychology is to destroy the essence of the functional view of life and reduce functions to new and inadequate structures. Finally, movement cannot be regarded as the cause of any of the antecedent conscious states. While the teleological view may have

some standing in other disciplines, it has no place in science; at the least it should be plainly labelled when introduced into a scientific discussion. It should be added that even after these inadmissible phases of the motor theory have been lopped off, the cardinal principles have not been touched. It still marks a great advance over the theories that preceded it.

If the earlier theory placed too much emphasis upon the sensory regions of the cortex and sensational qualities, this theory too much neglects them. Again it must be emphasized that all action is sensori-motor, that movement alone will not explain consciousness any more than will sensation alone. Movement grows out of sensation just as truly and inevitably as sensation gives rise to movement. Consciousness is made up of sensation understood with reference to movement, and of movement in the light of and under the control of sensation. More important than either sensation by itself or movement by itself is the fact that consciousness is always an organized system. This system is constantly growing with and by use, and to it every experience contributes. It is constantly receiving new contributions from sensory elements and each stage in its development is tested by action. That movements and thought both are ordinarily controlled by the system is the element of truth in the statement that meanings and related functions are motor. It is the system that both controls movement and gives meaning to the new experience. To assert that it is movement which gives meaning is but to substitute the result for the cause. Movement is one of the most frequent outcomes of organized experience and may be regarded as the sign or symbol of its presence. A complete explanation of any phase of consciousness can be neither in terms of sensation nor in terms of movement exclusively, but must include both. Both must be seen in the completeness of their reaction one upon the other, and of one sensori-motor circuit upon others. Above all there must be constant reference to the ever growing system of knowledge that develops out of sensation, that is tested by movement and which alone gives meaning to sensation and rational direction to movement.

EXPERIMENTAL STUDIES OF RHYTHM AND TIME.[1]

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I. QUALITATIVE LIMENS OR GRADES OF RHYTHM, AND THE DIFFERENCE LIMEN IN THE PERCEPTION OF TIME

In an earlier investigation of speech rhythm,[2] it was found that there were various qualities or degrees of speech rhythm, and that the advocate of the 'time theory' was incorrect in so far as he insisted on absolute periodicity as the *sine qua non* of rhythm. For in measuring phonograph records of recited verse, it was found that the listener was able to 'easily coördinate' the intervals or measures when they varied as much as 7 per cent. A variation of 12 per cent. rendered the rhythm vague, and a variation of 15 per cent. made it 'hard to rhythmize the impressions.' The rhythm was 'destroyed' when the variation reached one-third of a second, 'disturbed' when it reached one-fifth second, and 'well preserved' when it amounted to only one-tenth second. These results suggested the desirability of making a more

[1] The following research was carried out in the Princeton Laboratory in March and April, 1904, and a brief report of the same was presented at the Boston meeting of the American Psychological Association in 1905. Owing to various circumstances, chiefly the heavy teaching duties incident to normal school work, the publication of the complete data has been unduly delayed. It is regrettable also that my time is now so heavily taxed in the pursuit of other problems, that it will be possible to do little beyond writing a brief analysis of results and pointing out the more obvious deductions which follow. Unfortunately the same must apply to two or three articles which will follow in this series as soon as they can be prepared for publication. It is the intention, however, to reproduce the important original data in fairly complete tables, in order to make the material available for further study by future students of the problems of rhythm and time. Should time permit it is also intended to summarize at some future date the chief results in a final synthetical article, in which larger comparisons with the results of other investigators and wider conclusions will appear. Since, therefore, the articles in immediate contemplation will be analytical in character, the statements will be largely limited to a mere account of methods, findings and results.

[2] Wallin, 'Researches on the Rhythm of Speech,' Studies from the Yale Psychological Laboratory, IX., 69f, 128, 135, 1901.

careful determination of the amount of irregularity that can be introduced into rhythmical intervals without affecting, impairing or disrupting the rhythmical impression. The problem of the present investigation may, then, be stated as follows: how much may the time of rhythmical intervals be varied without perceptibly affecting the rhythm? And how many grades of rhythm can be differentiated? That is, how many steps or limens of rhythm can be produced by a constant increase of the time differences between the intervals by minimal steps? Is there a psychophysical constant of rhythmical variations?

The rhythmical material used to solve this problem consisted of the tones produced by the vibrations of a 500 D.V. fork (operated electrically) and amplified by a Helmholtz resonator (U_4). To break up the continuous tone made by the fork into alternating series of tones and silent intervals, a notched disc was constructed of heavy stiff non-warping paper board and made to revolve between the fork and the resonator in such a way that the notches would pass before the hole in the resonator. This disc was rotated by a Ludwig-Baltzar kymograph. To determine the regularity of the revolutions a 100 D.V. fork was made to trace a time line on a smoked paper pasted to the disc. Twenty-one of these time lines, measured from records made on two occasions, gave an absolute variation (M.V.) for each rhythmical measure or interval (60° of the disc) of .017 sec., and a relative variation of .0158 sec. (M.V./A. $A = 1.075$ sec.). The M.V. for each degree was accordingly .00028 sec.; and the M.V./A, .016 sec. Changes in the speed of rotation or irregularities were remarked only a few times by three subjects (D., W., Wa.), so that any slight variation in the mechanical regularity of the kymograph was probably negligible.

The disc was twelve inches in diameter. The grooves or notches were cut radially at the periphery. The shallow ones, giving the weak tones, were three eighths of an inch deep, while the deep ones, giving the loud tones, were six eighths of an inch deep. The depth of the latter was approximately the depth of the opening in the resonator, so that they

gave the maximum intensity of sound furnished by the ressonant tone. The relation of the intensity of the loud and weak tone was thus in the ratio of 2:1. There were six of these grooves in the disc, three deep and three shallow, the distance from the beginnings of the grooves being 60°. Each groove was 10° long, so that one sixth of the interval was audible. Accordingly one revolution of the disc furnished three loud and three soft tones, or three rhythmical measures, as indicated by the following scheme, in which the large dots indicate the loud tones: $\overset{1}{\bullet}\ .\ \overset{2}{\bullet}\ .\ \overset{3}{\bullet}\ .$
By reversing the disc on the kymograph, the iambus pattern (⏑—) could be obtained: $\overset{1}{:}\ \bullet\ \overset{2}{:}\ \bullet\ \overset{3}{:}\ \bullet$

The fork tones were transmitted to the subjects in an adjoining room by means of a rubber tube. One end of this was fitted over the resonator opening and the other end terminated in an improvised cardboard megaphone which was held close to the left ear by the subjects. The tones were easily audible. No sounds from the workings of the apparatus reached the listeners.

Two speeds or tempos were used. With one the rhythmical measure or interval length was 1.075 sec., and the duration of the tones .179 sec.; and with the other, 0.57 sec., and .095 sec., respectively. They were approximately twice as long in the one case as in the other.

Two methods of giving the tones were employed. In the 'continuous' method the subjects were allowed to listen to 18 successive tones or nine rhythmical intervals as the disc made three uninterrupted revolutions. In the 'discontinuous' method the tube was closed by squeezing together firmly the prongs of a pair of wing dividers during every alternate rotation of the disc. Therefore after the subject had heard six consecutive sounds, a period of silence intervened. This was succeeded by another period of sounds, then a period of silence, etc. Three subjects (S., D., Wa.), however, remarked that they got a faint impression of the sounds while the tube was closed. Any disturbance caused by this factor is probably negligible, so far as the general averages are concerned.

The regular variation in one of the interval lengths was effected by means of a 'variable' tone. One of the shallow grooves was made wider than the others, 28°. To the disc at this point was attached a sliding piece of zinc, containing a groove of standard width, namely, 10°. The moving of this slide shifted the sound, and thus made one interval shorter while the corresponding interval on the other side of the beat was made longer. The movement of the disc was made by hand, the amount being regulated by a degree scale drawn on the disc. The variable was always the last tone of each rotation in the trochee pattern (the sixth). The shifting followed the method of minimal change,[1] the amount of each change being usually one degree while the interval lengths were about equal (infrequently steps of one half degree were employed), and two degrees as the difference became pronounced. The subjects knew the direction of the change, but not the size of the steps. Each test always started with the tones objectively periodic. The differences were then gradually increased until the rhythm was considered destroyed (receding or ascending direction). Hereupon the movement was reversed (approaching or descending direction), the differences being gradually diminished until a perfect rhythm was again reached. The approaching series was usually started on a different reading than the last of the receding series.

The disc was always allowed to make three fourths of a revolution before the tube was opened (*i. e.*, before the subject was permitted to listen to the tones), in order to allow the kymograph to attain the right speed. The first sound released in the trochee pattern (— ⏑) was the first loud one after the variable; and in the iambic, the first weak one. The slide was displaced in one direction only, so that the interval vetween the fifth and sixth tones was lenghtened, and between the sixth and first shortened in the trochee pattern; and shortened between the fourth and fifth, and lengthened between the fifth and sixth tones in the iambic

[1] Woodrow's application of a similar method to the study of the rhythm and interval indifference points is accordingly more recent: 'A Quantitative Study of Rhythm,' Columbia University Contributions to Philosophy and Psychology, XVIII.

(the variable in this case being the fifth tone, as the loud tone came last in the measure).

The lengthening of one interval produced a compensatory shortening of the adjacent interval, except in the discontinuous method, trochee pattern, where no shortening occurred, because the sixth tone was followed by a period of silence, as already explained.

It is interesting to observe that the lengthened interval was noticed far more frequently than the shortened, as shown in detail in Table XIII. There was not a single subject who did not perceive that an interval had been lengthened, but the reverse was not true. Few of those who perceived the shortening did so at the beginning of the series; most of them noticed it only after the difference had become pronounced, or after it had been suggested by the experimenter. There was often a curious see-sawing between the two, in some cases probably due to the shifting of the attention from the one to the other. But as a rule as the intervals approached equality, the shorter interval tended to disappear. So far as the trochee pattern is concerned the explanation seems to be that the lengthened interval preceded the shortened (law of primacy), but this is not the case for the iambic, where the result is about the same.

It is possible that the sound of the variable, owing to the use of the zinc slide, differed somewhat from the other weak sounds. Two (M., McD.) remarked that the last beat 'clicked off' differently, or that it was pinched off or drawn out. A number reported slight irregularities with regard to the tones, such as that one of the loud tones seemed shorter once (F.), or longer (D., Wa.), or louder (D.); or a soft one seemed shorter (McD.) or longer (M.); or all the tones seemed fainter (C.), or some seemed to drag or stretch out more (Wh., W.). These were sporadic observations, and probably were mainly subjective effects. It is possible that on a few occasions the tones were all objectively fainter, owing to the occasional running down of the cell. On the whole, any objective difference between the variable tone and the two other soft ones was probably too small to affect the results adversely.

One sitting of about one hour was required to finish an experiment with each method for a given pattern and speed, expect that in the case of the shorter speed (0.57 sec.) both trochaic and iambic patterns were finished in one sitting. Each subject thus gave four sittings to the investigation. It was impossible to make more than one determination in each direction (ascending and descending series), although some of the steps were repeated while the subjects were asked to pay special attention to the rhythm or the time;[1] nor was it feasible to take the subjects at the same hour of the day. Five subjects (C., K., McD., M., S.) found it rather difficult to attend, on one or two occasions, owing to having slept poorly, being fatigued, depressed or hungry, or having just exercised; but with very few exceptions the observers testified that there was no difficulty in giving full attention to the experiment. The observers, besides Professor Warren, were students who had taken an introductory course in experimental psychology. The instructions given were uniformly: to sit erect, hold the megaphone close to the ear, to attend well, avoid haste and give disinterested responses. Uniform introspections were likewise called for, concerning the following items: the condition of the subject during the sitting, the regularity of the rotation, the preferred tempo and method, the fact as to whether the lengthening or shortening of the intervals was more apparent, the degree of the feeling of rhythm actually aroused by the tones, the nature of the processes which usually give the keenest perception of rhythm, the mood produced by the tempos or patterns, the differences produced by changing the variable upon the time and the rhythm, etc. To rule out the factor of time order, some subjects began with one method or pattern, and others with another, as shown in detail in the tables. But all the tests for the faster tempo, 0.57 sec., were begun after the experiments with the slower tempo were finished.

It is well to emphasize the fact that the attempt has been

[1] Occasionally steps were repeated three or four times without any such instructions. In such cases the results in the tables represent the average of three or four determinations.

made in this investigation to use tones which offer an *objective* rhythm; that is, regular recurrences of loud and soft tones—genuine objectively rhythmical groups. Not infrequently experiements on rhythm are based purely on periodicities which have little in common with rhythmical groups on the objective side. Whatever rhythm is felt is subjective. In this experiment, where objective rhythms were used, repeated enquiries were made as to whether or not the subjects actually experienced any rhythmical feeling. The results are embodied in Table XIV. In the 'preliminaries' (with the slower tempo only) the subject was merely told to listen in the megaphone. After a few rotations of the disc he was asked to describe his experience. This was repeated once or twice. A record was made as to whether the subject noticed that strong and soft tones were used, whether a rhythmical feeling was produced, and whether the tones seemed to come regularly. In later tests the subjects were asked to mention experiences or instruments which produce a rhythmical feeling of *like* intensity, and one of stronger intensity, the experiences which give them the most intense rhythmical impressions, and whether or not they found themselves using movements of any kind when listening to these tones.

An examination of this table will reveal a number of facts to which attention may be directed at this time. It appears from the preliminary introspections, made at the beginning of the first sitting, that all the subjects, except possibly a couple, experienced a genuine feeling of rhythm. This feeling apparently was no stronger, at the beginning, than the feeling of rhythm experienced when listening to a bell, an organ, a clock or metronome. It must be observed, however, that the feeling of rhythm received from the 0.57 sec. tempo was better than the rhythm from the slower rate. All except one of the subjects preferred the faster rate, eight because it gave a better feeling of rhythm, six because it made the tones shorter, sharper or more distinct, four because it made them sound louder, three because the changes were perceived easier, and two because the tones were easier to

follow.[1] It is probable therefore that practically all subjects (McD.'s rhythm sense seems undeveloped) experienced a rhythmical feeling, which grew stronger as the tests proceeded, even with the slower rate, and which were more intense, particularly with the faster rate, than the rhythmical impression produced by the regular tones of bells or metronomes, but in no case as strong as the beating of a drum. The keenest appreciation of rhythm, according to these reports, comes from dancing, band and orchestra music and drum taps. The additional fact, that no extensive use was made of movements in order to aid the rhythmical feeling, also indicates that the tones were adequate to produce rhythmical experiences.

We may conveniently consider the results in more detail under two headings.

I. The Differentiation of Rhythm into Qualitative Grades

On the basis of the results we are able to recognize five grades or qualities of rhythm in relation to the amount of irregularity introduced between the beats of the rhythmical series. These grades may be designated 'excellent,' 'good' (but somewhat shaky), 'medium' or 'fairly good,' 'poor' (or very jerky) and 'disrupted' rhythm. The average amount of time displacement or irregularity in terms of per cent. of the interval length for these five grades amounts, respectively, to the following: 6.36 per cent. (rhythm of grade I.), 8.53, 12, 14.5 and 17.8 per cent. (grade V.). These per cents. are based on the general averages for both speeds, patterns and methods (Table VI.). In absolute units of time the corresponding figures are: .0526, .0734, .0991, .1182 and .1488 sec.

[1]The *moods* produced or the *effects* of the two tempos upon the subjects were as follows:

0.57 sec.: Made me rather impatient (F.), agreeable mood (S.), spirited, slightly too fast (D.), lively impression (McD.), stirring (W.), a little too fast (M.), soothing, joyful (Wh.), tendency to beat, makes the rhythm pervade your body (K.), more restful, a little faster better (Wa.).

1.075 sec.: Soothing, gives time to think (F.), too slow (S.), soothing, soporific (D.), rather too slow (McC.), rather depressing, too slow (W.), soporific, too slow (Wh.) rather slow, soothing or indifferent (K.), rather too slow (Wa.).

These results have an important bearing upon the controversy between the so-called 'time' and 'accent' theories of rhythm. It is apparent that the time theory, which has been defended by Lanier, Gurney, Hauptmann, Westphal, Lobe, Herbart, Lotze, Schopenhauer, Bolton and Edwards,[1] is not strictly true. Absolutely periodic or regular occurrences are not essential to the appreciation of rhythm, although absolute regularity improves the quality of the rhythmic impression. To engender a feeling of rhythm always requires a certain amount of *periodicity;* but the margin of irregularity that may obtain is quite considerable, as seen above.

It is noteworthy that all of the subjects were able to group the rhythmical qualities into these five grades, rather than into merely three. It has been assumed that unaided psychological discrimination is capable of accurately making only a three-fold differentiation: equal, greater and less. It did sometimes happen, however, that a subject would skip a class; thus, Wa., though infrequently, tended to skip from the second to the fourth grade.

There was no means of determining the reliability of the subjects' judgments except the harmonious working out of the results and their own opinions. The subjects were asked at various times to state whether they felt certain of the grading, or whether the judgments were mere guesses. Two said they were very certain (S. and Wa.; except when near the point of equality for S., and except that it was sometimes difficult for Wa. to tell where he was); four that they were certain (C., K., S., Wh.), and two grew more certain as the experiment proceeded (D. and K.). C. was not sure in one particular test (1.075, continuous, — ⏑). It is believed that the degree of certainty is sufficient in practically all cases to render the results quite reliable.

The subjects were also asked, particularly during the last sitting (with the .057 sec. temp), to state which steps in the gradation were the easiest, and which the hardest, to determine. Three considered that the first grade (excellent,

[1] Wallin, *op. cit.*, 128.

I.) was the easiest (W., M., K.), two the fifth grade (F., W.), two from the fifth to the fourth, approaching direction (S., Wh.), and one each the fourth (D.), fourth and fifth (S.), and from the first to the second, ascending or receding direction (Wh.). Two each considered the following the most difficult judgments to pronounce: the third grade (F., W.), the second (D., K.), four and five (McD., Wa., ascending), and two to one, descending (S., Wh.); and one each the following: fourth (W.), fifth (M.), from one to two (S.), from two to three (Wa.), and from three to four (K.). It appears therefore that there is little agreement in this matter. Possibly the conclusion may be justified that, altogether, the first and fifth grades are differentiated most easily, and the third and fourth with the greatest difficulty.

Another point on which information was elicited was as to whether the grading was a matter of judgment or of feeling. Of the six subjects who reported upon this matter, four felt that they based their opinions upon feeling or a rhythmical sense (C., F., Wa.), two describing the feeling as a slight shock (S., Wa.), and two stating there was no sensation of pain present (C., D.). One did not know (D.), and one considered that it was a matter of judgment. Further enquiry was made as to whether it was easier to apprehend the time differences or the differences in the quality of the rhythm. The opinions of several subjects varied more or less in different tests, but seven reported at one time that the difficulty was about the same (C., D., K., M., W., Wa., Wh.); the two series, the time and the rhythm, ran parallel, the rhythm depending on the time (Wa.). Five considered that the rhythm problem was easier (C., D., F., S., Wa.), except for steps three, four and five for S., and except for the matter of arranging the rhythm into various classes for Wa. Only three considered the time problem easier (F., M. and K.; S. did so also, but only for the approaching direction). On the whole, then, it may be said that it was slightly easier to differentiate between grades of rhythmical qualities than to notice differences in time. These results taken together seem to point to the fact that time and rhythm do not rest

upon precisely the same basis. Rhythm is perhaps less a matter of judgment than of feeling or a rhythm sense. To occasion a genuine rhythmical reaction a movement in time must arouse those sensory processes, and motor reponses or physiological reactions which lie at the basis of the feeling of rhythm. Without an active functioning of the physiological or neural substrate, the rhythmical consciousness would consist of only a certain awareness of a quasi-rhythmical movement in time. The ideational arousal of certain revived processes—the images of the sensory experiences which underlie the rhythmical experience—would no doubt suffice to produce a semblance of rhythm, but it would lack the vividness and intensity which usually go with genuine first-hand experiences and which ordinarily are lacking in revived experiences.

Owing to the fact that the subjects were regularly making reports on the observed changes in both the time and the rhythm (especially at the beginning and end of the series), they were asked occasionally to disregard the time and attend exclusively to the rhythmical impression. The general results for the 'attention series' (which ran parallel with the regular series) are embodied in Tables XI. and XII. It is noticeable that the threshold values were smaller in sixteen cases when the attention was centered on the rhythm, and larger in only four cases. Attending to the grades permits of less variation in the intervals. The differences, however, in terms of absolute time, are merely nominal. If we compare the general averages of Table VI. with the similar averages of Table XI. (they are not precisely comparable because in the attention series the results are often available for one direction only, /\ or \/), we find that the gains for grades II. to V. amount to the following: .0012, .0011, .0080 and .0132 sec. But however small the improvement, if we concede that attending to emotional processes tends to dissipate them, then the results are in harmony with the view that rhythmical experiences are classifiable with sensory processes, and can therefore be rendered clearer by being attentively regarded. It follows as a consequence that the way to get pleasure from

rhythm is to attend to the impressions rather than the agreeableness of the experience—assuming, of course, that the movement is objectively regular.

It will be observed that the extremes found in this investigation for simple tones, 6.36 to 17.8 per cent., correspond fairly closely with those found in the earlier investigation in speech rhythm, 7 to 15 per cent. That is probably more than a coincidence. While the amount of variation needed to produce the various grades will differ, no doubt, to some extent with the character of the material (tones, speech, music, etc.) and the nature of the variation (here only one measure out of three was irregular), yet the ratios will probably be about the same. Such would be the presumption if there is a law regulating these changes—to this we will revert presently. It must be remembered, however, that in the case of the earlier speech rhythm investigation the initial category signified measures that could be 'easily coördinated' the last category those that were found 'hard to rhythmize' (instead of those in which the rhythm had been disrupted), and that no attempt was made in the speech investigation to determine the greatest number of possible grades of rhythm.

It will be appropriate to refer to the grades or steps of rhythm which we have found as *rhythm limens*. The number of limens is determined by the number of grades or steps between a perfect and disrupted rhythm. Apparently the rhythm limens are amenable to the Weber-Fechner psychophysical law, the mathematical constant being one third. For if we add one third of the value of a given limen to itself we obtain the value of the following limen. To this statement there are individual exceptions, and none of the empirical limens corresponds precisely to the theoretical; but the discrepancies are surprisingly small, as shown by the following figures. In these figures a minus sign indicates that the mathematical threshold is smaller, and the plus sign that it is larger than the empirically found limen. The discrepancies for rate 1.075 sec., continuous method, are, grade II., .0066 sec.−, for III., .0015+, for IV., .0178+, and for V., .0002 sec. +. For speed 0.57 sec. the corresponding deviations

are: .0001+, .0036−, .0051+, .0179+. The largest discrepancy is only .0179 sec. and the average of the eight only .0066. We find here, then, a striking extension of the psychophysical law to the field of rhythm and of esthetics.

When the results are averaged for the two speeds it is found that the theoretical values do not correspond very closely for grades IV. and V. Both are larger. This may indicate that the range to which the law applies in rhythm is quite limited, or otherwise that the subjects were overhasty in arriving at the determinations for the poorest rhythms.

The rhythm limens are not *sharp lines*, such as the thresholds of time differences. The moment the slightest difference in time is perceived we obtain a different limen. But the rhythm limens are extents, or time *spans*, having a certain breadth of time. Within these spans changes of time may be made without changing the grade or quality of the rhythm. The breadth of these rhythm thresholds varies considerably for different subjects, as may be seen by referring to the 'Range' columns in the tables. These refer to the intra-limen ranges, as distinguished from the inter-limen ranges, to which reference will be made in a moment. They have been calculated by subtracting the smallest from the largest single determination given by the subjects for any one grade of rhythm, so that, theoretically, they would represent 'neutral territory.' It is found that the average size of the rhythm *limen*, based upon the general average for the ranges, is, for the slower rate, .0499 sec., or 4.6 per cent. of the interval length; and for the faster tempo, .0222, or 4 per cent.; or 4.3 per cent. for both tempos (Table X.).

The ranges are fairly constant, the difference between the smallest and largest general averages being .0184 sec. for the slower tempo, and .0054 sec. for the faster, the M.V. for the two series being .0073 and .0017. They are less for the trochaic than the iambic pattern (in six out of ten cases), and for the discontinuous than the continuous method (except for grade III.).

The size of the intra-limen ranges may possibly also serve to indicate the degree of the subject's uncertainty. A very

large range would indicate that the subject is irregular, uncertain, or incapable of a fine appreciation of the differences between the various grades.

The rhythm limens do not merge directly into one another, but are separated by certain extents of time, which we may refer to as *inter-limen spans*. In the tables they are entered at the bottom as 'ranges between the grades.' They are obtained by subtracting the average of one grade from the average of the next higher. It is noteworthy that they are considerably smaller than the ranges within the grades. The average for the slower rate is .0336, or 3.1 per cent., and for the faster, .0151, or 2.7 per cent., which is 1.5 per cent. and 1.3 per cent. less, respectively, than for the corresponding ranges within the grades. This would seem to indicate that, while the limens are rather broad spans, they are quite sharply differentiated in consciousness. They do not vary much for the different grades (2.3, 3.1, 2.7 and 3.2 per cent.), but are relatively smaller for the faster tempo, as was the case with the previous ranges. There is no constant difference dependent upon pattern or method.

Are these inter-limen spans gaps, voids, in the rhythm curve? Probably not. They correspond to the imperceptible spaces between the successive sensation steps in the Weber-Fechner logarithmic curve of sensation, and thus parallel a well-known fact in this curve of sensation intensities.

A number of other minor results may be mentioned briefly. That there are considerable individual differences in the acuteness of discriminating grades of rhythm, can easily be seen by examining Tables I. to V. The acuteness is finer in the descending series, V, which approaches equality. In twenty out of twenty-five general averages for all grades these limens have a smaller value. This may be a practice effect, as the descending series always followed the ascending. Or the subjects may have been tardy in reaching a conclusion in both directions; this would tend to make the descending thresholds smaller.

Measured in terms of absolute time, the limens are all larger for the slower speed, but relatively to the length of

the measure (per cent. column) they are slightly smaller, except for grade V. In no case does the difference amount to more than 1.3 per cent., while the average difference for all grades in 0.6 per cent. That the limens should be smaller for the slower rate, in spite of the fact that all the subjects except one, as already remarked, preferred the faster tempo (because it heightened the feeling of rhythm, produced sharper sounds, etc.) may be due to the fact that the size of the minimal steps by which the tone was shifted was usually the same for the two tempos, namely, 1° at the beginning and 2° as the difference grew larger. Only occasionally were .5° steps used with the faster tempo. Vierordt found the keenness sensibly finer for the faster of two speeds. The difference for six series with the rate of 100 per minute was 23.5 per cent., as against 29.9 per cent. with the rate of 72 per minute.

The rhythm limens are smaller with the continuous than the discontinuous method, except for grade IV., the differences for the successive grades, based upon the per cents, being: 1.1 per cent., 0.9 per cent., 1.5 per cent., .1 per cent. (larger, for grade IV.), and .3 per cent., or an average of 1.2 per cent., for all grades. These results are based on the corresponding series, the trochaic pattern of the continuous and discontinuous methods. The experimental results correspond with the introspections. Seven subjects preferred the continuous method (only three the discontinuous), because it fixed the rhythm better (Wa.), or made it easier to apprehend (D., Wh., S.), or it held the attention better. The lenghening followed by a shortening produced a disturbance that was more easily apprehended than the disturbance caused by the lengthening alone (Wa.). One had to start the rhythm afresh every time with the discontinuous method. On the other hand, the discontinuous was considered better because there was not the same danger of losing the interval (F., W.), although one regarded the continuous better from the standpoint of the rhythm.

The limens are also somewhat smaller in the trochaic than the iambic pattern, the differences between the successive

grades being: .0142 sec., .0110, .0159, .0113 and .0050, or an average of .0115 sec. There is only one exception among the grades of the two tempos; viz., grade V., 0.57. Here again the introspections corroborate the experimental findings. All of the seven subjects who were questioned preferred the trochee, because the trochee rhythm was better or could be judged more easily (by five, F., D., McD., Wh., K.), or it was easier to attend to (F., Wh.), or it was preferred normally (F; but Wa. prefers iambic in poetry), or it was more soothing (D.); or the iambic was harsh and grating (K.), or it was tiring (F.), or unbalanced (S.) and unfamiliar (F.). The results point strongly to the conclusion that a trochee type of rhythm gives greater satisfaction than an iambic—a fact suggesting an important application in the field of musical and poetical esthetics.

II. The Difference Thresholds in the Perception of Time

As has already been said, the subjects were required to render both a time and rhythm judgment, particularly near the point where the intervals were about equal. They were requested to state after each liminal change at the beginning of the ascending and at the close of the descending series, whether or not any difference was noticed in the periodicity of the beats and in the quality of the rhythm. They also reported on the point at which the measures in the descending series appeared equal; *i. e.*, the point where the beats again came regularly. The results for the time judgments are given in Table XV.

For purposes of comparison the most reliable threshold value is probably the average of the first perceived difference (difference limen) of the ascending series, the last perceived difference of the descending series and the following equality judgment of the descending series. The constant errors incident to the particular direction of movement probably neutralize one another and thus disappear in the average for the three determinations. That an error of anticipation is present is indicated by the facts that four of the general

averages are larger and only one smaller for the ascending (∧) than the descending (∨) series. Accordingly the difference threshold was anticipated in both series. Naturally the equality threshold is smaller than either the ascending or descending thresholds, but there is one exception in the former series. It is noteworthy, however, that there is quite a number of individual exceptions: for some subjects the equality thresholds are sometimes larger than the difference thresholds, particularly in the ascending series. It is a matter of surprise that this should ever occur in the descending series, for here the point of equality necessarily followed the last difference limen. The explanation is that the subjects, on a few occasions, reversed their equality judgments and rendered a difference judgment without rendering a second equality judgment before the series of minimal changes was finished. This indicates a certain degree of subjective uncertainty or irregularity, which would probably have disappeared after practice, and which evidently would have occurred less frequently had the task of the subjects been restricted to rendering only a time judgment, instead of both a time and rhythm judgment. As the *time* problem was regarded as merely incidental to the *rhythm* problem, for purposes of comparison, check and control, no attempt was made to carry out a complete systematic determination of the temporal threshold. Besides their interest for comparative purposes, the results for time discrimination have an independent value under the conditions in force.

The following points summarize the more important results:

I. The difference thresholds for *time* are invariably *smaller* than the first rhythm limen (grade I.). The difference is not very large; in no case more than .0202 sec., or 1.9 per cent., and in four cases only .0145 sec., .0091 sec., .0072 sec. and .0018 sec. It thus appears that the time and rhythm thresholds are not identical; they do not precisely correspond. A change in time is apprehended before a change in the quality of the rhythm is felt. Moreover, there is this essential difference between the two thresholds: the time threshold is a

sharp edge or line, while the rhythm threshold is a broad range or extent, whose average duration, as already seen, amounts to 4.3 per cent. of the interval length (p. 112). That is to say, just as soon as the slightest change in the regularity or periodicity of the beats is apprehended, we have arrived at a temporal limen; as soon as another minimal difference is perceived we have a second time limen. In the case of rhythm, however, a certain appreciable increment must be added to the time limen before any effect on the rhythm is experienced. The perception of time and rhythm are, no doubt, closely related processes (Woodrow[1]), but they are not identical, even in the elemental perception of time differences. A 'temporal grouping' is obviously involved in rhythmical grouping, but the temporal grouping due to 'grouping by segregation' —*i. e.*, by increasing the temporal value of the preceding interval—is secondary. The primary and perhaps the only essential conscious datum in the experience of the rhythmizing subject is the fairly regular alternation of intensity differences (centroids[2]).

II. The difference threshold is relatively smaller for the longer interval. The size of the thresholds, based on the averages for the corresponding method (continuous), is .0323 sec. for the 0.57 sec. tempo, which is 5.6 per cent. of the length of the interval; and .0526 sec. for the 1.075 sec. tempo, which is 4.8 per cent. of the interval length. Relatively to the interval length, therefore, the limen for the longer interval is 0.8 per cent. smaller. In the rhythm series the corresponding difference was somewhat smaller, 0.6 per cent. This difference between the rates, as already explained (p. 114), is perhaps partly due to the fact that the minimal steps were in most cases absolutely the same for both tempos. This would tend to make the thresholds relatively larger for the faster speed. At the same time, since the subjective rhythm feeling was keener for the faster tempo and since this was the preferred tempo, we have here an additional indication that the time estimations and rhythm

[1] Woodrow, *op. cit.*, 50.
[2] Wallin, *op. cit.*, 9 ff.

appreciation are not identical and do not run precisely parallel.

III. If we compare the two methods, using the same pattern (— ⏑), it appears that the threshold for the continuous method is slightly smaller, the difference amounting to .0073 sec., or .7 per cent. The particular method is of less consequence in the estimation of time than in the differentiation of rhythm. For the rhythm judgments the limens were 1.2 per cent. smaller for the continuous than the discontinuous method (p. 114). Time differences can be appreciated relatively better than rhythm differences in short series. To secure the rhythmical swing requires a longer repetition of beats; and until a felt rhythmical swing is obtained the discrimination of grades of rhythm is not maximally effective. The continuous method, however, is superior in both the time and rhythm series.

IV. As to patterns, the limens are slightly smaller for the trochaic (— ⏑) than the iambic (⏑ —) type of measure. The difference is about the same in the two tempos, namely, .0046 sec., or .8 per cent. for 0.57 sec., and .0074 sec., or .77 per cent. for 1.075 sec. Here again the superiority of the trochee is seen to be more patent in the rhythm than the time series (p. 114).

It would be worth while to determine more fully whether the advantage of the continuous method and trochaic pattern arises primarily from the element of time or the element of rhythm. So far as our experimental results and introspections go, the rhythm is the determining factor.

V. As to the *size* of the time limens: the smallest, relatively to the interval length, is 4.5 per cent. (or .0489 sec. for speed 1.075 sec.); and the largest 6 per cent. (or .0346 sec. for speed 0.57 sec.). For the continuous method the thresholds are 5.67 per cent. for .057 sec. (or .0323 sec.), and 4.8 per cent. for the 1.075 sec. speed (or .0526 sec.). In three cases they are identical, 5.2 per cent. This is also the general average for all the tables, so that we may place the value of the differential limen in the estimation of time under similar conditions at 5.2 per cent. of the interval length. This deter-

mination falls within the limits of Vierodt's results. He found a difference limen of 2.5 per cent. with a tempo of 100 beats to the minute and 6.9 per cent. with a tempo of 42. These differences were needed to give 70 per cent. of correct answers.

Explanation of terms in the tables:

Grade I., II., III., etc. These represent five successive steps of increasing disintegration of the rhythmical unity. I. is relatively perfect rhythm; V., relatively disintegrated rhythm.

∧ *and* ∨. These signs indicate the directions in which the changes in the length of the variable interval are made by minimal steps. ∧ is the ascending or receding series of steps: it begins with equality, so that each step increases the difference. ∨, descending or approaching series; the return movement, toward equality; each step decreases the difference.

−1, −2 and −3, attached to the initials of the first subject in a different time series. The numerals indicate the time order of the various patterns for each subject. Thus for subject D., speed 1.075 sec., the experiments occurred in the following order: first sitting, continuous method, — ⌣. Second sitting, discontinuous method, — ⌣. Third sitting, continuous method, ⌣ —. In all cases the experiments with the 1.075 sec. speed were finished before work was begun with the 0.57 sec. speed.

'*Range*' (*within the grades*). The 'range' is the distance between the smallest and largest determinations given by the subject for each grade, irrespective of the direction (∧ or ∨). It represents the difference between the shortest and longest individual determination, and thus indicates indirectly the degree of uncertainty of the subject's judgment. The averages of these ranges are based upon the number of counts, and not the number of subjects.

Range between the grades. These ranges correspond to the differences between the *averages* of the various grades. Thus the difference between the averages of I. and II. is .0428—.0318=.0110, or 2 per cent. of the length of the interval (0.57 sec.).

Continuous method. Here the disc revolved three times, thus permitting the subject to hear eighteen consecutive tones, nine loud and nine low, or six standard measures and three variables.

Discontinuous method. Here the tones were audible during each alternate revolution only.

Qualitative Limens of Rhythm

Table I

Duration of measures or intervals: 0. 57 sec. (time between the beginning of the tones).
Duration of tones: .095 sec. (duration of each degree of arc: .0095).
Pattern: 2-beat, —⌣. Continuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
S.–2	.0285	.0332	.0475	.0736	.0570	.0475	.1045	.0855	.0285	.1187	.1045	.0190	.1425	.1235	.0190
C.	.0380	.0237	.0380	.0570	.0427	.0285	.0855	.0617	.0285	.1045	.0760	.0285	.1140	.0950	.0190
W.	.0190	.0285	.0285	.0332	.0475	.0285	.0475	.0665	.0190	.0712	.0760	.0285	.0950		
M.	.0665	.0570	.0665				.0950	.0855	.0285	.1235				.1140	
Wa.	.0190	.0285	.0285	.0285	.0380	.0095	.0380	.0475	.0095	.0475			.0570		
Ave.–2	.0342	.0342	.0418	.0481	.0463	.0285	.0617	.0693	.0228	.0926	.0855	.0253	.1021	.1108	.0190
D.–1	.0095	.000	.0095	.0190	.0142	.0095	.0380	.0380	.0380	.0475	.0617	.0380	.0760		
F.		.0190	.0190	.0095	.0285	.0190	.0285	.0380	.0190	.0475	.0475	.0000	.0570		
McD.	.0475	.0570	.0570	.0570	.0665	.0095	.0855	.0855	.0190		.1045		.1140		
Wh.	.0237	.0237	.0285	.0380	.0429	.0095	.0570	.0617	.0095	.0855	.0855	.0190	.1045		
K.	.0380	.0475	.0475	.0522	.0570	.0095	.0760	.0665	.0190	.1045	.0760	.0285	.1140	.0950	.0190
Ave.–1	.0297	.0294	.0323	.0351	.0418	.0114	.0570	.0579	.0209	.0712	.0750	.0214	.0931		
Ave.–1 and 2	.0319	.0318	.0370	.0416	.0440	.0199	.0593	.0636	.0218	.0819	.0802	.0233	.0976	.1029	.0190
Ave. ∧ and ∨ Range	.0318 = 5.5 per cent. Between grades I. and II., .0110, or 2 per cent.			.0428 = 7.5 per cent.			.0614 = 10.8 per cent. Between II. and III., .0186, or 3.1 per cent.			.0810 = 14.2 per cent. Between III. and IV., .0196, or 3.4 per cent.			.1002 = 17.6 per cent. Between IV. and V., .0192, or 3.4 per cent		

TABLE II

Duration of intervals and tones: same as in Table I.
Pattern: 2-beat, ⌣ —. Continuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
F.–2	.0475	.0190	.0285	.0570	.0427	.0285	.0665						.0760	.0665	
McD.	.0570	.0760	.0190	.0855	.0427	.0380	.1235	.1045	.0190	.1424	.1140	.0285	.1615	.1330	.0285
Wh.	.0238	.0238	.0285	.0332	.0427	.0285	.0617	.0617	.0095	.0855	.0807	.0190	.1045		
K.	.0475	.0475	.0000	.0617	.0570	.0095	.0950	.0665	.0380		.0760		.1140	.0950	.0190
D.	.0285	.0475	.0190	.0427	.0617	.0385	.0617	.0807	.0285	.0855	.0950	.0190	.0950		.0190
Ave.–2	.0409	.0428	.0190	.0560	.0510	.0286	.0817	.0783	.0293	.1045	.0914	.0222	.1102	.0981	.0222
S.–1	.0380	.0380	.000	.0522	.0570	.0190	.0807	.0807	.0095	.1045	.0950	.0095	.1140		
C.	.0475	.0285	.0190	.0570	.0380	.0238	.0665	.0522	.0190	.0807	.0665	.0190	.0950	.0760	.0190
W.	.0570	.0475	.095	.0665	.0570	.0095	.0858	.0665	.0190	.0950	.0855	.0190	.1045		
M.	.0570	.0475	.095	.0665	.0617	.0095		.0760		.0974	.0950	.380			
Wa.	.0285	.0285	.0000	.0712	.0427	.0190					.0570		.0665		
Ave.–1	.0456	.0380	.0078	.0627	.0513	.0181	.0776	.0688	.0158	.0944	.0798	.0216	.0950	.0760	.0190
Ave.–1 and 2	.0432	.0404	.0134	.0593	.0511	.0233	.0796	.0735	.0225	.0994	.0856	.0219	.1026	.0870	.0206
Ave. ∧ and ∨ Range	.0418 = 7.3 per cent. Between I. and II., .0134 = 2.3 per cent.			.0552 = 9.6 per cent. Between II. and III., .0213 = 3.7 per cent.			.0765 = 13.4 per cent. Between III. and IV., .0160 = 2.8 per cent.			.0925 = 16.2 per cent. Between IV. and V., .0023 = 4 per cent.			.0948 = 16.6 per cent.		

For explanation of terms, see p. 119.

TABLE III

Duration of measures or intervals: 1.075 sec. (time between tones).
Duration of tones: .179 sec. (duration of each degree of arc: .0179).
Pattern: 2-beat, — ⌣. Continuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
D.–1	.0716	.0537	.0179	.0984		.0179	.1343	.1029	.0716		.1790		.2148		
W.	.0716	.0179	.0716		.0447	.0537	.0895	.1074	.0358	.1074	.1611	.0537	.1790		
K.	.0179	.0358	.0179	.0537	.0626	.0358	.0895	.0985	.000	.1253	.1163	.0358	.1790		
Ave.–1	.0537	.0358	.0358	.0760	.0536	.0358	.1044	.0999	.0358	.1163	.1521	.0447	.1909		
S.–2	.1074	.1074	.0000	.1656	.1253	.0895		.1611		.2685	.2148	.0895		.2689	
McD.	.0626	.1074	.0537	.1134	.1879	.1253	.1969		.1253						
M.	.0537	.0358	.0358	.1253	.0656	.0895	.0895	.1074	.0358	.0895	.1600	.1074	.2506	.1611	.0895
Ave.–2	.0745	.0835	.0298	.1348	.1262	.1014	.1432	.1342	.0805	.1790	.1874	.0984	.2506	.2150	
F.–3	.0179	.0358	.0179	.0447	.0537	.0179	.0805	.0805	.0179	.1074			.1432	.1074	.0358
C.	.0537	.0179	.0358	.0805	.0447	.0537	.1074	.0716	.0358		.0895		.1253	.1074	.0179
Wh.	.0179	.0358	.0179	.0537	.0626	.0358	.0984	.0884	.0179	.1432	.1253	.0179	.1790	.1611	.0179
Wa.	.1432	.0805	.0716	.1790	.1163	.0716	.2327	.1611	.0895	.2864			.2685	.2148	.0716
Ave.–3	.0582	.0425	.0358	.0895	.0693	.0447	.1297	.1004	.0403	.1342	.1074	.0179	.1790	.1477	.0358
Ave.–1, 2, 3	.0621	.0539	.0338	.1001	.0830	.0606	.1258	.1115	.0522	.1432	.1490	.0537	.2068	.1813	
Ave. ∧ and ∨ Range	.0580 = 5.4 per cent. Between I. and II., .0335, or 3.1 per cent.			.0915 = 8.5 per cent. Between II. and III., .0271, or 2.5 per cent.			.1186 = 11 per cent. Between III. and IV., .0275, or 2.6 per cent.			.1461 = 13.6 per cent. Between IV. and V., .0479, or 4.4 per cent.			.1940 = 18 per cent.		

TABLE IV

Duration of intervals: 1.075 sec. (time between tones).
Duration of tones: .179 sec. (duration of each degree of arc: .0179).
Pattern: 2-beat, ⌣—. Continuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
F.–1	.0716	.0179	.0537		.0537	.0358	.1253	.0716	.0895	.0895	.1253	.0358	.1790		
S.	.0895	.0984	.0179	.1253	.1522	.0716	.1790	.1969	.0358	.2237		.0358	.2685	.2327	.0358
Wh.	.0447	.0358	.0358	.0805	.0716	.0179	.1074	.1074	.0358	.1432	.1432	.0358	.1969		
Wa.	.0179	.0716	.0537	.0718	.1325	.1432	.1491	.2148	.1074	.2327		.0358	.2864		
Ave.–1	.0559	.0559	.0403	.0925	.1025	.0671	.1408	.1477	.0671	.1723	.1342	.0358	.2327	.2327	
C.–2	.1790	.1074	.0716		.1342	.0179				.2148	.1785	.0537	.2506		
K.	.0537	.0716	.0358	.0835	.1253	.0716	.1432	.1253	.0716	.1700		.0179	.2237	.1879	.0537
Ave.–2	.1163	.0895	.0537	.0835	.1297	.0447	.1432	.1253	.0716	.1924	.1785	.0557	.2371	.1879	.0537
D.–3	.0000	.0179	.0179	.0477	.0358	.0537	.0626	.0716	.0537	.1253	.0984	.0358	.1611		
McD.	.0776	.1253	.1074	.1208	.1611	.1432		.1969		.2506					
M.	.0895	.1253	.0358	.1313	.1611	.0537	.1969						.2506	.1969	.0716
W.	.0776	.0537	.0716	.0626	.0716	.0179		.1074		.1253			.1790	.1432	.0358
Ave.–3	.0612	.0805	.0582	.0906	.1074	.0671	.1297	.1253		.1671	.0984	0.358	.1969	.1700	.0537
Ave.–1, 2, 3	.0778	.0753	.0507	.0889	.1132	.0596	.1379	.1328	.0641	.1773	.1370	.0418	.2222	.1968	.0477
Ave. ∧ and ∨ Range	.0765 = 7.1 per cent. Between I. and II., .0245 = 2.2 per cent.			1.010 = 9.3 per cent. Between II. and III., .0343 = 2 per cent.			.1353 = 12.5 per cent. Between III. and IV., .0218 = 2 per cent.			.1571 = 14.6 per cent. Between IV. and V., .0524 = 4.8 per cent.			.2095 = 19.4 per cent.		

TABLE V

Duration of intervals and tones: 1.075 sec. (time between tones).
Duration of tones: .179 sec. (duration of each degree of arc: .0179).
Pattern: 2-beat, —⌣. Discontinuous method.

Subject	Grade I			Grade II			Grade III			Grade IV			Grade V		
	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range	∧	∨	Range
C.–1	.0537	.0537	.0000	.0716	.0716	.0358	.1074	.1074	.0358	.1432			.1790	.1611	
McD.	.0626	.0179	.0537	.0984	.0806	.0537	.1432	.1163	.1790	.1790					
M.	.0895				.0716		.1790	.0268	.1611	.1074	.0984	.0179	.1432		
Ave.–1	.0686	.0358	.0268	.0850	.0746	.0447	.1432	.0835	.1253	.1432	.0984	.0179	.1611	.1611	.0179
F.–2	.0895	.0537	.0358	.1074	.0805	.0358	.1253	.1253	.0358				.1611		
D.	.0000	.0268	.0537	.0358	.0537	.0895	.0805	.1074	.0716		.1432		.1432		.0358
Wh.	.0358	.0537	.0179	.0626	.0805	.0358	.1074	.1074	.0179	.1432	.1253	.0179	.1790	.1611	.0179
Wa.	.1253	.1163	.0179	.1432	.1611	.0179		.1969	.0358	.2148	.2506	.0716	.2864	.2685	.0179
W.	.0358	.0358	.0000	.0537	.0805	.0537	.0805	.0805	.0179				.1611	.1253	.0358
Ave.–2	.0573	.0572	.0250	.0805	.0913	.0467	.0984	.1235	.0358	.1790	.1730	.0447	.1862	.1850	.0268
S.–3	.1790	.1074	.0716	.2327	.1432	.0895	.2864	.1969	.1253				.3043	.2506	.0537
K.	.0537	.0716	.0179	.0805	.0984	.0358	.1074	.1253	.0179	.1342	.1432	.0179	.1700		.0179
Ave.–3	.1163	.0895	.0447	.1566	.1208	.0626	.1969	.1611	.0716	.1342	.1432	.0179	.2371	.2506	.0358
Ave.–1, 2, 3	.0807	.0608	.0322	.1074	.0955	.0513	.1462	.1227	.0776	.1521	.1382	.0268	.1948	.1989	.0268
Ave. ∧ and ∨ Range	.0707 = 6.5 per cent. Between I. and II. .0307 = 2.8 per cent.			.1014 = 9.3 per cent. Between II. and III., .0330 = 3 per cent.			.1344 = 12.5 per cent. Between III. and IV., .0107 = 1 per cent.			.1451 = 13.5 per cent. Between IV. and V., .0517 = 4.8 per cent.			.1968 = 18.3 per cent.		

Tables of General Results

Table VI

Average Values for All Grades

Speed 1.075 *sec.*

	Grade I	Grade II	Grade III	Grade IV	Grade V
	Sec. %	Sec. %	Sec. %	Sec. %	Sec. %
Continuous method, — ⏑	.0580 = 5.4	.0915 = 8.5	.1186 = 11.	.1461 = 13.6	.1940 = 18.
Continuous method, ⏑ —	.0765 = 7.1	.1010 = 9.3	.1353 = 12.5	.1571 = 14.6	.2095 = 19.4
Ave.	.0672 = 6.2	.0962 = 8.9	.1269 = 11.7	.1514 = 14.1	.2017 = 18.4
Discontinuous, — ⏑	.0707 = 6.5	.1014 = 9.4	.1344 = 12.5	.1451 = 13.5	.1968 = 18.3
Ave.	.0684 = 6.33	.0979 = 9.06	.1294 = 12.	.1497 = 13.9	.2001 = 18.5
		Speed 0.57 *sec.*			
Continuous, — ⏑	.0318 = 5.5	.0428 = 7.5	.0614 = 10.8	.0810 = 14.2	.1002 = 17.6
Continuous, ⏑ —	.0418 = 7.3	.0552 = 9.6	.0765 = 13.4	.0925 = 16.2	.0948 = 16.6
Ave.	.0368 = 6.4	.0490 = 8.01	.0689 = 12.1	.0867 = 15.2	.0975 = 17.1
General Ave. (both speeds)	.0526 = 6.36	.0734 = 8.53	.0991 = 12.	.1182 = 14.5	.1488 = 17.8

Table VII

Comparison of Methods

Continuous.

	Grade I	Grade II	Grade III	Grade IV	Grade V
	Sec. %	Sec. %	Sec. %	Sec. %	Sec. %
Speed 1.075, — ⏑	.0580 = 5.4	.0915 = 8.5	.1186 = 11.	.1461 = 13.6	.1940 = 18.
Speed 1.075, ⏑ —	.0765 = 7.1	.1010 = 9.3	.1353 = 12.5	.1571 = 14.6	.2095 = 19.4
Speed 0.57, — ⏑	.0318 = 5.5	.0428 = 7.5	.0614 = 10.8	.0810 = 14.2	.1002 = 17.6
Speed 0.57, ⏑ —	.0418 = 7.3	.0552 = 9.6	.0765 = 13.4	.0925 = 16.2	.0948 = 16.6
Ave.	.0520 = 6.3	.0726 = 8.8	.0979 = 11.9	.1589 = 14.6	.1496 = 17.9
		Discontinuous.			
Speed 1.075, — ⏑	.0707 = 6.5	.1014 = 9.4	.1344 = 12.5	.1451 = 13.5	.1968 = 18.3

TABLE VIII

COMPARISON OF PATTERNS

— ⌣

	Grade I		Grade II		Grade III		Grade IV		Grade V	
	Sec.	%	Sec.	%	Sec.	%	Sec.	%	Sec.	%
Continuous, 1.075	.0580	5.4	.0915	8.5	.1186	11.	.1461	13.6	19.40	18.
(Discontinuous, 1.075)[1]	(.0707	6.5)	(.1014	9.4)	(.1344	12.5)	(.1451	13.5)	(.1968	18.3)[1]
Continuous, 0.57	.0318	5.5	.0428	7.5	.0614	10.8	.0810	14.2	.1002	17.6
Ave.	.0449		.0671		.0900		.1135		.1471	
⌣ —										
Continuous, 1.075	.0765	7.1	.1010	9.3	.1353	12.5	.1571	14.6	.2095	19.4
Continuous, 0.57	.0418	7.3	.0552	9.6	.0765	13.4	.0925	16.2	.0948	16.6
Ave.	.0591		.0781		.1059		.1248		.1521	

[1] Not counted in average, owing to difference of method.

TABLE IX

COMPARISON OF RANGES BETWEEN THE GRADES

Speed 1.075 *sec.*

	Between Grades I. and II		Between II. and III		Between III. and IV		Between IV. and V	
	Sec.	%	Sec.	%	Sec.	%	Sec.	%
Continuous, — ⌣	.0335	3.1	.0271	2.5	.0275	2.6	.0479	4.4
Continuous, ⌣ —	.0245	2.2	.0343	3.2	.0218	2	.0524	4.8
(Discontinuous, ⌣ —)[1]	(.0307	2.8)	(.0330	3)	(.0107	1)	(.0517	4.8)[1]
Ave.	.0290	2.6	.0307	2.8	.0246	2.3	.0501	4.6
Speed 0.57 *sec.*								
Continuous, — ⌣	.0110	2	.0186	3.1	.0196	3.4	.0192	3.4
Continuous, ⌣ —	.0134	2.3	.0213	3.7	.0160	2.8	.0023	.4
Ave.	.0122	2.1	.0199	3.4	.0178	3.1	.0107	1.9
General Ave.		2.3		3.1		2.7		3.2

General averages for all grades of speed 1.075 sec.: .0336 sec., or 3.1 per cent of 1.075. M.V. = .0065.

General averages for all grades of speed 0.57 sec.: .0151 sec., or 2. 7 per cent. of 0.57. M.V. = .0037.

M. V. refers to the M. V. between the measurements for the different grades.

[1] Not counted in the average, owing to difference of method.

TABLE X

COMPARISON OF RANGES WITHIN THE GRADES (INTRA-LIMEN RANGES)

(The ranges represent the difference between the longest and shortest times which occur in either of the series ∧ and ∨. The averaging is based on the number of counts in a column and not the number of subjects.)

Speed 1.075 *sec.*

	Grade I	Grade II	Grade III	Grade IV	Grade V
	Sec.	Sec.	Sec.	Sec.	Sec.
Continuous, —⌣	.0338	.0606	.0522	.0537	.0358
Continuous, ⌣—	.0507	.0596	.0641	.0418	.0477
(Discontinuous, —⌣)[1]	(.0322)	(.0513)	(.0776)	(.0268)	(.0268)[1]
Ave.	.0422	.0601	.0581	.0477	.0417
Speed 0.57 *sec.*					
Continuous, —⌣	.0370	.0199	.0218	.0233	.0190
Continuous, ⌣—	.0134	.0233	.0225	.0219	.0206
Ave.	.0252	.0216	.0221	.0226	.0198

[1] Not counted in the average.

Averages for all grades, speed 1.075 sec. = .0499, or 4.6 per cent. M.V. = .0073 sec.
Aves. for all grades, speed 0.57 sec = .0222, or 4 per cent.

TABLE XI

ATTENTION SERIES

The determination of the rhythmical limens when attention is centered on the rhythmical impression.

Speed 1.075 *sec.*

	Grade I	Grade II	Grade III	Grade IV	Grade V
Continuous, —⌣	.0716[1]	.0887	.1248	.1288[2]	.1700
Continuous, ⌣—	.0716	.0939	.1342	.1387	.2028
Discontinuous, —⌣	.0805	.0822	.0885	.1454[2]	.1611[2]
Ave.	.0745L.	.0882G.	.1158G.	.1374G.	.1779G.
Speed 0.57 *sec.*					
Continuous, —⌣		.0506	.0570	.0760	.0918
Continuous, ⌣—		.0665	.1045	.0902	.0950
Ave.		.0585G.	.0807G.	.0831G.	.0934G.

[1] Individual count; not an average.

[2] Average for ∧ only. The above table gives only the general averages for the subjects from whom results were obtained. L. = loss of keenness. G. = gain.

Table XII

Comparison of losses and gains between the attention series, and the series in which no specific instruction was given to direct the attention to the ryhthm. G.= a gain in the fineness of rhythmical discrimination in the attention series; L=loss (based on general averages or the averages for ∧ and ∨ when no general average is available).

			0.57 sec. Continuous, —⌣		Summary	
I	II	III	IV	V	G.	L.
	L.	G.	G.	G.	3	1
			Continuous, ⌣—			
	L.[1]	L.[1]	G.	G.	2	
			1.075 sec. Continuous, —⌣			
L.[1]	G.	L.	G.	G.	3	1
			Continuous, ⌣—			
G.	G.	G.	G.	G.	5	
			Discontinuous, —⌣			
L.	G.	G.	L.	G.	3	2
					16	4

[1] Not based on averages

Table XIII

Table showing whether the lenghtening or shortening of intervals produced by displacing the tone was more noticeable.

Subj.	Speed 1.075 sec.						Speed 0.57 sec.					
	Continuous, —⌣			Continuous, ⌣—			Continuous, —⌣			Continuous, ⌣—		
	L.	S.	B.	L.	S.	B.	L.	S.	B.	L.	S.	B.
F.	Y	[1]				Y	Y	[2]		Y[3]		
S.	Y			Y	Y	Y[4]	Y[5]			Y		
D.	Y[6]			Y			Y	Y[7]		Y[3]		
C.	Y			Y[9]						Y[10]		
McD.		[11]		Y	[7]		Y[12]			Y	Y	
W.	Y[13]				Y[11]		Y[14]					Y
M.	Y			Y[15]					Y[16]	Y[17]		
Wh.	Y			Y			Y	Y				Y[19]
K.	Y			Y	Y[20]		Y[21]	Y			Y[22]	
Wa.	Y			Y[23]			Y[18]			Y[11]		

L., lengthening. S., shortening. B., both. Y. yes. (This indicates that the lengthening, or shortening of the intervals was noticed, or both, according to the column in which Y appears.

[1] Merely a suggestion of a shortened interval.

[2] Perceived it only when looking for it.

[3] Lengthening noticed, but cannot tell very well.

[4] Noticed only the longer at first, then both, then the shorter better, then both equally well, and then only the shorter.

[5] First only the longer, then both. Shorter at one time more distinct.

[6] First the longer,then both, then the shorter was clearer, then the longer.

[7] Noticed the shorter when attending to it.
[8] First the longer, then shorter also, then the longer only or better.
[9] First only the longer, then the shorter, then only the longer.
[10] First the longer, then both (longer clearer).
[11] Mostly.
[12] First the longer, then both, then the longer.
[13] First the longer, then both, then the longer usually.
[14] First the lengthening, then both, then only the lengthening.
[15] First the longer, then the shorter also; latter grew more distinct.
[16] Both, but shorter vague toward the close.
[17] First the longer noticed, then both (but the longer better), then the longer only.
[18] Noticed the lengthening first.
[19] Varied at first, then noticed the longer, and then both.
[20] Noticed the shorter when suggested, then more easily.
[21] Noticed the longer, then the shorter (but not so well).
[22] Shorter first, then the longer also, but the shorter was usually clearer.
[23] Noticed the lengthening first, then both, then the shortening more clearly, then both equally well then the shortening less, then only the lengthening.

TABLE XIV

Introspections on the rhythmical impressions produced by the loud and soft resonator tones.

Subj.	Preliminary introspections (speed 1.075 sec.).			Preliminary and later introspections.		Later reports on what instrument or experience gives the strongest rhythmical feeling.[1]	Did you have any method of keeping or aiding the rhythmical impression?
	Were weak and strong beats felt?	Any rhythmical feeling?	Were the beats regular?	What instrument gives a similar rhythm?	What a stronger rhythm?		
F.	Yes.	None: too slow (later very good).	Yes.	Organ.	Drum taps.	Dancing.	No.
S.	?	A suggestion. Rather slow	Yes.	Bell.	Drum taps, much better.	Two-step, or puffing of engine.	No. Beat time only in attention.
D.	Yes.	Yes.	Yes.	Noise of railway train or dentist's saw.	Drum taps.	Dancing, good waltz.	No.
C.	Yes.	Yes (later very good)	Yes.	Bell or clock.		Dancing. Music.	None for 0.57 sec. Beat with feet and hands once for 1.075.
McD.	Yes.	Yes (?)[2]	Yes.		Dactylic hexameter.		None noticed.
W.	Yes.	Yes.	Yes.	Metronome.	Drum taps.	Music. Quickstep. Band.	Tapped with finger sometimes.
M.	Yes.	Yes, when suggested.	Yes.	Tolling of large bell.	Drum taps.	Dancing.	Moved hand sidewise.
Wh.		Yes, second time (later very good).	Yes	Bell.	Drum taps, much superior.	Drum or trombone in band. Rapid quickstep.	
K.	Yes.	Yes.	Yes, slowing toward close.			Band music. Seeing dancers and beating time with the music.	Beat time with hand and foot.
Wa.	Yes.	Yes.	Yes.	Loud clock.		From operetta with orchestra, and seeing players sway.	Did not notice. Needed no aid with 0.57.

[1] In a later experiment on preferred tempos with the metronome, the same question was asked six other subjects, with the following results: (1) Orchestra, good martial music or bugle call. (2) Band music (good rhythm from poetry also). (3) Drum taps. (4) Metronome.

[2] Apparently has a poor sense of rhythm.

TABLE XV

TIME JUDGMENTS.

The difference thresholds in the perception of time. The results are all for grade I., and represent the general averages for all the subjects from whom data were obtained.

Speed 0.57 sec., continuous, — ⌣.

∧	∨	≧	Ave.	
.0289	.0327	.0285	.0300, or 5.2 per cent.	(Results for the Rhythm Threshold.)
			Same, ⌣ —.	
.0356	.0399	.0285	.0346, or 6.0 per cent	(.0418, or 7.3 per cent.)
			Speed: 1.075 sec.	
			Continuous, — ⌣.	
.0514	.0502	.0487	.0501, or 4.6 per cent.	(.0580, or 4.6 per cent.)
			⌣ —.	
.0418	.0714	.0526	.0553, or 5.1 per cent.	(.0765, or 7.1 per cent.)
			Discontinuous, — ⌣.	
.0544	.0642	.0500	.0526, or 5.2 per cent.	(.0707, or 6.5 per cent.)

∧ ascending series. ∨, descending series. ≧, equality judgments in descending series.

EXPERIMENTAL STUDIES IN JUDGMENT

Judgments of the Comic

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Reactions to comic situations are notoriously dependent on individual temperament, mood and circumstance. So much is this true that few attempts to control these variable factors experimentally or to measure in any way the subjective element in the response to the comic have been reported. One of the most striking things about such reactions is the important rôle played by adaptation to the stimulus. There is a decided change in the reaction whenever the element of novelty, surprise or shock is eliminated, or whenever the climax is in any way anticipated or remembered. But the mere fact that these factors are subjective or highly variable is no reason why they should not submit to measurement by appropriate methods. Nothing would seem to be more subjective and variable than judgments of scientific eminence, of the relative beauty of picture postals, the legibility of handwriting, family resemblance in penmanship or the persuasiveness of advertisements. Yet all these have yielded to methods of measurement and the coefficients derived have been sometimes objectively verified. Thus the writer has found that subjective judgments of the relative persuasiveness of advertisements actually check up with the keyed results of those advertisements when they appeared.

The variable elements in judgments of the comic fall into two chief groups:

1. *Variabilities in the observer.*—No two individuals are supposed to react in quite the same way to the same comic motive. Associations leading into remote quarters of personal experience, recent happenings and present apperceptive

[1] The writer was assisted in the conduct of the experiment and in the computation of the 'waxing' and 'waning' results reported in section II. by Miss E. V. Dunnett, A. B. Barnard, '10.

attitudes most diverse in kind, emotional complexes varying widely in color and complexity, seem nowhere else to be so potent as here. One individual never wearies of a favorite jest which for others soon loses its flavor. Another is perennially amused by puns which bore his friends; some of us always 'see the funny side' while others never seem able to 'catch the point.' Even the fact of racial differences, so difficult to detect in most traits, is apparent here.

2. *Variabilities in the comic situation.*—Every-day observation shows that some jokes lose their relish immediately after the first hearing while others will endure a considerable number of repetitions before adaptation takes place or even will improve with increased familiarity. There are differences in speed, in the number of repetitions required, as well as in the final degree of adaptation, which may be easily measured, analyzed and balanced against each other. The semi-metaphysical classics have had much to say concerning the process of adaptation, but these statements have been usually spun out of exceeding fine yarn. The only strictly experimental ventures into the field of the comic, to the writer's knowledge, are the questionnaire of Hall and Allin[1] and the tests of Martin[2] by the various psychophysical methods. Neither of these papers throws any clear light on the questions proposed in the present inquiry. The purpose of this study is to seek answers to some or all of the following questions concerning the two groups of variabilities outlined above.

I. Judgments of the comic:

1. What differences are found among a group of observers, in the apperception, appreciation and evaluation of a given series of comic situations?

2. What variability does the same observer show from time to time in his reactions to the same motive?

3. How does the average variability of the same observer compare with that of a group of observers?

4. Is the consistency of an individual in any way corre-

[1] Hall and Allin. On Tickling and Laughing. *Amer. Jour. of Psychol.*, **9**, Oct., 1897, pp. 1–41.

[2] Martin, L. J. Psychology of Esthetics. *Amer. Jour. of Psychol.*, **16**, 1905 pp. 35–117.

lated with his own agreement with the group average? Is his consistency in any one task characteristic of his performance in a different task?

5. What relation does the character or quality of the motive bear to the degree of variability in these cases?

6. Does the judgment of the comic display any peculiar characteristics? What contribution to the general theory of judgment can such an investigation make?

II. The psychology of adaptation to the comic:

1. Do all jokes decrease in funniness with repetition?

2. If so, do they all grow stale together or do some become chestnuts more quickly than others?

3. Or are there perhaps (*a*) waxing jokes, in which each repetition provokes an even greater appreciative reaction, (*b*) waning jokes which decline steadily in affective value, and (*c*) static ones, which undergo no transformation in merit. What are the chief 'categories' of the comic, and what is the behavior of each type in the chestnut process?

III. Can such experimental investigation contribute in any way toward the 'theory of the comic' which hitherto for the most part has been treated only in an analytical and introspective way?

Method and Procedure

The comic situations were in this experiment afforded by a series of thirty-nine jokes, ranging rather widely in character and merit, culled from a collection to which several individuals contributed their choicest specimens. The attempt was made to secure, as far as possible, 'uncirculated' jokes, so that each observer would begin at an approximately identical point of adaptation. It need hardly be remarked that this condition was not rigidly fulfilled. Tradition asserts that there are only five distinct jokes in the world, but for our purpose 10 chief categories were chosen as comprising the principal types disclosed by an analysis of the whole collection and several representatives were selected for each category. The classification seems to cover most of the distinct types pointed out in the analytical treatises. In order to classify the jokes fairly, ten disinterested observers independently

sorted the group of 39 under the 10 headings, and in the final interpretation of the results with respect to the adaptation process the majority of these 10 votes decided the category under which the joke was thrown (see Table IX.). The jokes were type-written on cards of the same size, and in order to leave the interpretation of the situation entirely to the reader, the titles, often suggestive of one interpretation rather than another, were omitted.

The plan adopted for securing the judgments was a modification of the order of merit method so successfully employed by Cattell,[1] Sumner,[2] Norsworthy,[3] Wells,[4] Downey[5] and Thorndike.[6] The present modification, which may be designated the *group method* as contrasted with the *strict order method* possesses several advantages which justify its further development. Instead of arranging the material in strict order of merit the observer placed them in 10 piles, according to their 'degree of funniness.' In the first pile were placed the superior jokes, in the tenth the poorest ones, while the intermediate piles represented gradations of merit from best to poorest. No instructions were given as to the amount of difference represented by these successive piles, nor as to the number of cards to be placed in each.[7]

Ten observers took part in the experiment, all of whom were women, students in the Barnard laboratory, with one and a half year's work in psychology. When the average position of each card for the ten observers was calculated, the 39 jokes

[1] Cattell, J. McKeen. A Statistical Study of American Men of Science. *Science*, **24**, Nov. and Dec., 1906. See also his Statistics of American Psychologists. *Amer. Jour. of Psychol.*, **14**, 1903, pp. 310–328.

[2] Sumner, F. B. A Statistical Study of Belief. PSYCHOL. REV., **5**, 1898, pp. 616–631.

[3] Norsworthy, N. The Validity of Judgments of Character. Essays Philosophical and Psychological in Honor of William James, 1908, p. 551.

[4] Wells, F. L. On the Variability of Individual Judgment. Essays Philosophical and Psychological in Honor of William James, 1908, p. 511.

[5] Downey, J. E. A Preliminary Study of Family Resemblance in Handwriting, Psychol. Bulletins, Univ. of Wyoming, No. 1, 1910.

[6] Thorndike, E. L. Handwriting. *Teachers College Record*, **11**, No. 2, 1910, p. 57.

[7] Just what differences in degree this classification would show, and whether or not a Weber's law principle is present in the discrimination of these affective stimuli, would be an interesting topic for investigation.

could be arranged in a strict order of merit according to their respective averages. The advantages of this group method are several.

It is much quicker than the strict method, less fatiguing and monotonous to the observer, yet correlates closely with results from the same observers by the strict order method. Further, the method gives opportunity to observe any changes in value of the group as a whole. Thus by multiplying the number of cards in a given group (say 7) by the position of that group (say number 9) and adding these products for all ten groups a figure is obtained which gives some measure of the total value of the series for a given individual or group. Now if the cards are arranged a second, third, fourth, etc., time by the same observers, these sums will indicate the change in total value of the series during the successive trials (see Plate III.). This figure is of course not in any sense an absolute measure. It is conditioned by shifts in the individual's standard of value, by his personal variability of judgment, by the variation in standard from individual to individual, and by the fact that no card can be thrown higher than the first nor lower than the last pile. Nevertheless it affords an interesting and suggestive index of the total series behavior which the strict order method cannot yield. It will be shown later that the M.V. (mean variation) in such experiments always bears a constant ratio to the number of places into which the objects are to be sorted, so that the relative variability is the same here as in the strict method.

There may be, in the *group method*, a certain tendency to arrange stimuli according to qualitative or type resemblance, which might to a degree disturb the judgment of merit,—a tendency, that is, to put all puns in the same pile, etc. But there is no evidence in the results that such an inclination has in any way operated. Moreover the tendency is just as strong, in the *strict order method*, to put qualitatively similar stimuli in the same region of the scale. Thus Wells found that in arrangements of picture postals according to preference there was a tendency to place near each other cards bearing similar scenes, color schemes, etc. It is conceivable

that even in arranging individuals with respect to scientific eminence, contiguity in space or similarity of field or method may operate as a more or less significant associative factor in determining relative position. But since these factors also help determine the individual's actual judgment of merit, they need not be supposed to warp that judgment in any undesirable way.

In the present experiment each of the 10 observers arranged the cards five successive times, the trials being a week apart. This plan thus gave data for investigating the variability of the group, of the individual, of the total value of the series and of the behavior of each card under the influence of repetition. Both Wells and Downey have shown that a week is ample time for the elimination of any great disturbance through the memory factor in the successive trials.

Results

Results will be treated under three headings: First, with respect to judgment; second, with respect to the 'chestnut' process; third, with respect to the theory of the comic.

Table I. shows the average value and the M.V. of each card (numbered in a random order) for the 10 observers in the five successive trials, and indicates also the final position of each joke in each trial. Adding each column of average positions gives for each trial the total value for the series and averaging the M.V. column gives the average M.V. of the 10 observers for each of the five trials. Thus the total value of the series amounts to 214.1 points in the first trial, and drops in each following trial, through 216.5, 230.7, 235.0, to 239.9 in the fifth. That is to say, the jokes as a whole wane in value, assuming that the average standard of the group remained approximately constant throughout. The shift is not so great, however, as we might expect. Since the observer was instructed to have always ten piles, the jokes could not all begin in pile 1. There would be at least one in each of the other nine piles, making the highest possible number of points (with 30 cards in pile 1 and one card in each pile from 2 to 10) 84. Nor could the cards all fall to the bottom pile, since there

must needs be one remaining in each of the other nine, so that the lowest possible limit would be 240. Thus the maximum possible fall would be from 84 to 340 points, or a loss of 256 points. The actual drop is only from 214 to 240, or 26 points, approximately one tenth of the possible fall. This slight change is probably due to the fact that the cards originally in pile 10 could never be thrown lower. Yet those in the piles above, while becoming weaker in their laughter-provoking quality, preserved their superiority to the pile below and could not be assimilated with them. This means then that the general standard of the individual would fall from trial to trial, the lowest group (10) representing each time an absolutely lower value. This drop would extend itself all the way up the scale, so that the jokes which remain on a level throughout the experiment really drop, those rising do not rise so much as they seem to, while those dropping fall more rapidly, with respect to absolute value, than they appear to. The total waning coefficient does not then measure the absolute change in value of the group, but it is nevertheless a significant figure in comparison of group with group or individual with individual.

The average M.V.'s for the successive trials show no change in magnitude, remaining always between 2.1 and 2.3 places, or .22 the total number of positions. The significance of this ratio will be referred to later.

No.	1st Trial			2d Trial			3d Trial			4th Trial			5th Trial		
	Av.	M.V.	Pos.	Av.	M.V.	Pos.	Av.	M.V.	Pos.	Av.	M.V.	Pos.	Av.	M.V.	Pos.
1	6.4	2.60	30	6.8	2.44	31	7.3	2.24	34	7.4	1.60	34	7.6	1.96	32
2	5.2	2.24	14	6.6	2.12	28	6.1	2.10	21	5.2	2.34	12	6.8	2.04	23
3	5.5	3.10	18	4.9	2.32	13	5.4	2.70	13	4.9	2.21	8	5.9	2.52	16
4	5.6	3.12	19	7.0	2.20	35	7.6	1.68	36	7.0	2.40	31	7.1	1.80	28
5	5.4	2.20	16	5.0	1.80	14	6.7	1.76	27	5.5	2.30	13	5.9	1.71	15
6	5.9	1.90	22	3.6	3.04	2	4.3	2.56	6	3.8	1.66	4	4.5	2.40	5
7	7.0	2.00	37	7.7	1.82	38	7.3	2.10	33	8.6	1.96	39	8.2	1.80	39
8	3.8	2.20	4	4.0	2.30	5	3.9	2.10	4	5.8	1.84	16	4.9	2.68	10
9	3.2	1.40	2	5.2	2.44	15	5.8	2.10	17	6.4	3.64	22	6.5	2.20	20
10	6.4	1.52	28	6.1	1.70	24	5.6	2.80	16	6.1	2.85	19	6.2	2.06	18
11	6.9	1.98	35	7.3	1.70	37	6.9	1.43	30	7.5	1.90	35	8.1	1.50	37
12	4.3	2.08	9	4.2	2.08	7	5.1	2.32	10	5.2	2.08	10	4.0	1.80	3
13	6.6	2.08	32	6.3	1.76	25	6.9	1.80	31	6.8	2.62	28	7.7	1.63	33
14	6.3	1.84	27	5.8	2.08	21	5.8	2.56	19	6.1	2.72	20	7.2	2.40	30
15	6.4	2.20	21	5.7	2.70	19	6.2	1.40	22	6.8	1.80	27	6.9	1.92	25
16	6.2	2.60	25	6.6	2.76	30	6.2	2.96	23	5.7	3.26	15	6.5	3.10	21
17	7.2	2.36	38	7.7	1.88	39	8.3	1.78	39	8.3	2.50	38	8.2	1.56	38
18	5.2	2.44	15	6.3	2.30	26.5	6.8	2.26	29	7.3	2.30	33	7.2	2.52	31
19	6.8	2.08	34	6.8	2.12	34	6.8	2.04	28	6.9	2.10	29	6.5	2.00	19
20	4.2	1.88	7	4.3	1.56	8	5.5	2.20	15	5.8	2.14	17	4.8	1.84	7
21	4.0	1.80	5	4.6	2.40	11	4.8	1.60	9	4.3	1.62	5	4.2	1.84	4
22	4.8	2.50	11	6.0	1.40	23	4.6	1.52	8	5.6	1.80	14	5.2	1.96	11
23	2.2	1.48	1	2.0	1.20	1	1.9	0.90	1	2.6	1.66	1	2.2	1.12	1
24	6.1	2.34	24	6.8	3.04	33	7.1	3.18	32	6.5	3.10	25	7.0	2.80	26.5
25	5.0	2.40	12	5.8	2.74	22	6.4	2.20	24	6.5	2.20	23	6.8	2.63	24
26	5.5	2.60	7	4.4	1.88	10	5.4	2.60	12	2.9	2.13	2	4.8	2.00	8
27	3.7	2.84	3	3.9	3.16	4	3.8	2.98	2	3.7	2.44	3	3.3	2.22	2
28	5.7	3.30	20	5.7	2.76	29	5.8	2.74	18	6.7	2.80	26	6.6	2.92	22
29	4.8	2.40	10	5.2	3.40	17	5.3	2.90	11	4.6	2.40	7	5.2	3.44	12
30	4.1	2.52	6	4.1	1.90	6	3.9	1.72	3	5.2	2.24	11	4.8	2.20	9
31	6.5	3.20	31	6.6	2.40	29	6.5	2.10	25	7.7	1.38	37	7.8	1.84	35
32	5.0	3.20	13	4.7	2.18	12	4.5	2.70	7	6.4	2.92	21	5.5	3.00	13
33	6.9	2.94	36	6.6	2.60	32	7.5	2.20	35	7.0	2.80	32	7.0	2.80	26.5
34	7.2	2.72	39	7.0	2.60	36	7.9	3.17	37	6.9	2.80	30	7.8	2.38	36
35	5.8	2.40	21	5.4	2.48	18	5.9	2.50	20	6.5	2.40	24	5.9	2.72	17
36	4.2	2.04	8	3.7	1.56	3	4.2	1.86	5	4.5	2.10	6	4.6	1.49	6
37	6.7	2.50	33	6.3	2.30	26.5	8.0	1.80	38	7.5	2.40	36	7.7	2.17	34
38	6.0	1.80	23	5.3	1.76	16	5.5	2.20	14	4.9	2.83	9	5.7	1.98	14
39	6.2	2.64	26	4.4	1.68	9	6.6	2.02	26	5.9	2.50	18	7.1	2.46	29
	214.1 Sum	2.3 Average.		216.5 Sum.	2.2 Average.		230.9 Sum.	2.2 Average.		235.0 Sum.	2.3 Average.		239.9 Sum.	2.1 Average.	

Variability within the Group

Likes and Dislikes.—If the cards be arranged in a final order of merit for each trial and the M.V.'s of the best cards compared with those of the poorest, that is, if the M.V.'s of the top and bottom of the series be compared, the members of the group are found to agree more closely at the top than at the bottom. Table II. gives the M.V. for the first and last ten places in each of the five trials. Inspection shows two facts. First, that the M.V. for the top groups, taken either by 5's or 10's, is less than for the lower. Thus the average M.V. for places 1–10 is 2.03 compared with 2.22 for places 30–39. The M.V. of places 1–5 is 1.97 compared with 2.09 for places 34–39.

Table II

Av. M. V.'s, 10 Observers, 5 Trials

Pos.	Trial 1	Trial 2	Trial 3	Trial 4	Trial 5
1	1.48	1.20	0.90	1.66	1.12
2	1.40	3.04	2.98	2.12	2.22
3	2.84	1.56	1.72	2.44	1.80
4	2.20	3.06	2.10	1.66	1.84
5	1.80	2.32	1.86	1.62	2.40
6	2.52	2.40	2.56	2.10	1.49
7	1.88	2.08	2.70	2.40	1.84
8	2.04	1.56	1.52	2.21	2.00
9	2.08	1.68	1.60	2.83	2.20
10	2.40	1.88	2.32	2.08	2.68
—					
—					
—					
—					
30	2.60	2.76	1.43	2.80	2.40
31	3.20	2.12	1.80	2.40	2.52
32	2.08	3.04	3.18	2.80	1.96
33	2.50	2.44	2.10	2.30	1.63
34	2.08	2.12	2.24	1.60	2.17
35	1.98	2.40	2.20	1.90	1.84
36	2.94	2.20	1.68	2.40	2.38
37	2.00	1.70	3.16	1.38	1.50
38	2.36	1.88	1.80	2.50	1.56
39	2.72	1.82	1.78	1.96	1.80

Second, this difference becomes smaller with each repetition, the differences between the M.V.'s of 1–5 and 34–39 being successively .46, .23, .21, .13, .05, and between the M.V.'s of 1–10 and 30–39, being .39, .24, .17, .10, .01. Generalizing we may say that in the beginning individuals agree more

closely on the good than on the poor, but that with successive repetitions this difference disappears (see Table III.).

TABLE III

AVERAGES FROM TABLE II

	1	2	3	4	5	Average
Av. 1–5	1.94	2.23	1.91	1.90	1.87	1.97
Av. 34–39	2.40	2.00	2.12	2.03	1.82	2.09
Difference	+ .46	− .23	+ .21	+ .13	.05	
Av. 1–10	2.06	2.08	1.97	2.11	1.96	2.03
Av. 30–39	2.45	2.32	2.14	2.21	1.97	2.22
Difference	+ .39	+ .24	+ .17	+ .10	+ .01	

This first relation seems to be a usual one in judgments of this subjective character,—of preference, beauty, persuasiveness, etc. Thus in Wells' study of picture postals, although the author does not call attention to the fact, the figures yield the following result. For places 1–5 and 45–50, the M.V.'s are much alike, being respectively 8.7 and 8.5. For places 1–10 the M.V. is 8.5 while for 40–50 it is 10.2. For 1–15 it is 9.5 as against 10.3 for places 35–50, etc. In a yet unpublished study by the writer, of the judgments of twenty men with respect to the persuasiveness of 50 advertisements the same relation holds, the average M.V. for places 1–10 being 9.8 as against 10.8 for places 40–50.

This seems to be a not uninteresting point in the psychology of judgment, the significance of which is not clear. Wells finds that for repeated trials by the same individual the reverse situation holds, the same individual agreeing closer at the bottom of the scale than at the top, and the suggestion is made that this may mean that we are more certain of our dislikes than of our preferences. Giving the present relation a somewhat analogous interpretation, it may mean that although a single individual may be more certain of his antipathies, a group of individuals will resemble each other more in their preferences than in their aversions.

Or the relation may mean simply that we attend to things possessing positive quality. That here where the expression of the judgment is in terms of preference we attend

more strongly to the end in which our preferences really lie. But that this is not true for all individuals will be later pointed out. Dearborn[1] finds judgments of unlikeness easier to make than judgments of similarity, and Downey finds some evidence for the same relation, although the average of her results confirms the statement of Wells. But the judgment of preference is qualitatively different from the judgment of resemblance, the one being based on feeling-tone, the other on more restricted perceptual factors.

Another possible interpretation of the data is that the differences between the superior cards, at the top of the scale, are greater than those of the mediocre at the bottom. This was clearly shown by Cattell to be the case in judgments of scientific achievement. Thus "The figures show that the average differences[2] between the chemists who are in the first tenth are about eight times as great as between the chemists toward the middle of the list and about twelve times as great as between the chemists toward the bottom of the list" But there are at least three reasons for believing that there is considerable change in attitude when the same observer turns from arranging men according to merit to arranging simple stimuli according to affective tone. The difference lies in the fact that part way down the scale, in the latter case, the expression of judgment changes from terms of decreasing preference into terms of increasing positive dislike, whereas probably few scientists who would get into a total group would be rated as positively bad, the judgment being expressed rather in terms of more or less merit. Arrangements of scientific merit resemble the scale of sensation intensities, varying always in terms of degree, while arrangements of preference resemble the gradation of feelings from the positive pole through a region of indifference to a decided negative pole.

In the second place the suggestion that the smaller variability in the upper ranges depends on objective differences in

[1] Dearborn, G. V. N. Notes on the Discernment of Likeness and Unlikeness. *Jour. of Phil., Psychol., and Scientific Methods*, **7**, 1910.

[2] Measured inversely by the size of the probable errors and directly by the difference in grade.

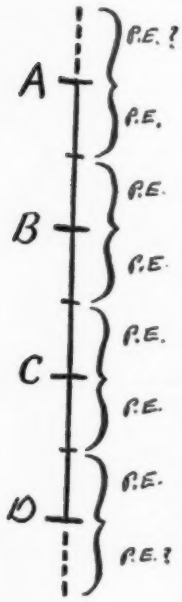
the stimuli is contradicted by the fact that in the successive arrangements by the same individual four of the ten observers were more consistent in the lower range than in the upper, and this would hardly be expected if the differences between the cards in this lower range were actually smaller than in the upper. Furthermore if something like Weber's law holds for judgments of affective tone as well as for sensation intensity, differences in the upper range would have to be greater in order to yield equal variability, and considerably greater if the variability is still smaller. The whole question of this closer group agreement in the upper ranges seems to merit further investigation and especially, the tendency of the differences to become uniformly smaller in successive trials.

Another striking relation brought out by the comparison of various order of merit arrangements of stimuli on the basis of such affective factors as preference, beauty, persuasiveness, funniness, etc., is the constancy of the ratio of the average M.V. for the series as whole to the number of possible positions in the range. If by M.V. we designate this average variability and by P the total number of positions in the scale then M.V./P is, with various kinds of material, with different groups of observers and with a widely ranging value for P, usually .20, and with high reliability. The following table exhibits this relation in such material as the writer has at hand.

TABLE IV

Material	Trait	Observer	P	M.V.	M.V./P
1. 4 advertisements	Persuasiveness	10 men	4	.8	.200
2. 5 advertisements	Persuasiveness	10 men	5	.98	.196
3. 39 jokes	Funniness	10 women	10	2.2	.220
4. 10 advertisements (av. of 4 sets)	Persuasiveness	10 women	10	2.3	.230
5. 10 advertisements (av. of 3 sets)	Persuasiveness	20 mixed	10	2.5	.250
6. 20 advertisements (av. of 2 sets)	Persuasiveness	50 mixed	20	4.3	.215
7. 20 photographs	Various traits	10 women	20	3.6	.180
8. 39 jokes	Funniness.	10 women	39	8.03	.205
9. 50 appeals	Strength.	20 women	50	10.5	.201
10. 50 picture postals (Wells)	Beauty.	10 mixed	50	10.7	.201

That is to say, the M.V. is always about one fifth of the total number of possible places, or the P.E. (probable error) assuming a normal distribution, about .168 or about one sixth of the range. The evidence seems to the writer too strong to permit of explanation in terms of mere coincidence. Of course if the material had been the same throughout, the only variable being the number of places into which it was sorted, this is just what we might expect, for the relative P.E. would remain constant, the absolute P.E. depending on the fineness of the grades of distinction. But we have here eight distinct sets of material, judged in termsof a considerable range of traits, by widely differing groups of observers, both as to sex, training, interest and number. The only constant factor is that the judgment is always based on the affective reaction to the stimulus. And we find that in every case the probable error is approximately one sixth of the range. (It would probably be slightly larger if it were not for the fact that the end error tends to reduce the variability of the extreme upper and lower positions.) Assuming that the M.V.'s were equal in all parts of the range (and they do not vary greatly), and allowing a P.E. in both directions from both the upper and lower extremes, the total range would then be divided into four sections, each separated from its neighbor by the respective P.E.'s, somewhat as follows. This would mean that, so far as the average judgment of the group of observers is concerned, there are only four distinct grades of difference or merit in the material, only four shades of distinction on which the group would, in the long run, agree, these grades corresponding to the sections lying about *A*, *B*, *C* and *D* as central tendencies.



This situation is curiously analogous to that disclosed in judgments of the same observer, where practice shows that about four distinctions of certainty, clearness, etc., are all

that can be comfortably and accurately made. The same thing that holds for the variability of the individual holds for the variability of the group. And the fact that the law holds for such different kinds of material and traits argues an interesting resemblance between the judgments involved in such affective discriminations.

The size of this ratio M.V./*P* would become smaller as the material came to be selected so as to disclose more pronounced or more objectively measurable differences. Thus in judgments of resemblance of penmanship which are supposedly more directly perceptual and objectively verifiable in kind, Downey finds M.V.'s which, if arranged as below, according to the range of possible positions, would yield an M.V./*P* value of about .163, or a probable error of about .130, meaning that while there are only about four clearly marked grades of beauty, funniness, persuasiveness, etc., there are about five clearly marked degrees of resemblance.

TABLE V

VARIABILITY OF JUDGMENTS OF SIMILARITY (DOWNEY)

P	M.V.	M.V./*P*
20	3.31	.165
34	5.33	.157
37	6.22	.168

Average M.V./*P* = .163

This ratio M.V./*P* can then be used as a reliable index of the objective character of judgments and with greater accuracy than the crude M.V. employed by Wells. Using this ratio the objectivity of his three classes of judgments would be,—preference .201, colors .086, and weights .141, showing that the judgments of weight order were more subjective than those of color order, thus reversing the order assigned by Wells.

INDIVIDUAL VARIABILITY

Turning now from the variability of the group to the successive judgments of the individual, several questions suggest themselves;

1. Does the individual, as the group, judge the upper range positions with greater regularity than the lower?

2. Is there any correlation between the individual's consistency in his successive judgments and his agreement with the average of the group?

3. Is there such a thing as general consistency, that is, does the individual whose successive judgments in this experiment show little variability judge as uniformly under other circumstances?

4. Is there any correlation between the individual's relative position in the group with respect to consistency or with respect to agreement with group average in this experiment, and the same measures in another experiment, with different material? In other words, is the individual who is variable in judging one class of material also variable in judging other material, and is his approximation to the group average in one case any index of his position for other material?

TABLE VI

Av. M.V.	Ellison	Mahon	Mordecai	Denike	Gerstein	Maschmedt	Pratt	Bishop	Schlesinger	Hart	Av.
1– 8	.59	1.08	.86	1.42	.43	1.93	.43	.43	.72	.50	.84
9–18	.77	1.68	1.05	1.84	1.21	2.48	1.41	.137	1.15	.96	1.39
18–25	1.20	2.27	1.62	3.00	1.35	1.81	1.86	1.14	1.15	1.05	1.64
25–32	1.19	1.96	1.43	1.94	1.20	.82	1.93	1.07	.86	1.35	1.37
32–39	.68	1.27	1.56	1.12	.57	.12	1.12	.34	.47	.72	.78
Av. 1–39	0.88	1.65	1.30	1.86	0.95	1.43	1.35	0.87	0.87	0.92	

TABLE VII

Observer	Size of M.V.		Correlation with Av.	
	M. V.	Pos.	*V*	Pos.
Bishop	.87	1	.296	9
Schlesinger	.87	2	.432	7
Ellison	.88	3	.320	8
Hart	.92	4	.477	6
Gerstein	.95	5	.700	1
Mordecai	1.30	6	.536	5
Pratt	1.35	7	.283	10
Maschmedt	1.43	8	.602	3
Mahon	1.65	9	.551	4
Denike	1.86	10	.664	2
	$r = -.49$ with P.E. = .16			

Table VI. gives for each individual the M.V. of the five trials for the different sections. These figures were secured by computing the final average of each card for the individual, arranging the cards in a final order of merit according to these averages and then calculating the M.V. of each position in the five trials. These M.V.'s for the eight positions in each section were then averaged to get the average M.V. for the section. The average of these gives, finally, at the bottom of each column, the average M.V. of the individual for all 39 sections. Comparison of the column shows that in six cases the M.V.'s are smaller for the upper and in four cases for the lower ranges, the average for the 10 observers being practically identical for corresponding sections, perhaps somewhat less for the lower ranges,—.84 for 1–8 as against .78 for 32–29, and 1.39 for 9–18 as against 1.37 for 25–32. So far as this difference is genuine it confirms the finding of Wells that the same individual is, on the average, more certain of the judgments in the lower than in the upper sections, differing in this respect from the judgments of the group.

Table VII. shows the consistency of each individual as measured by her average M.V., the correlation of her own average arrangement with the average arrangement of the group of 10 observers, also the position of each observer in the group with respect to both these measures. The correlation between these two main columns is −.49, indicating that a large individual variability or personal inconsistency is correlated directly with approximation to group average. This provides an interesting contrast with Wells' result for picture postals. In his experiment the individual whose personal consistency was greatest correlated most closely with the group average, and this led to the suggestion that he who knows his own mind best is likely to be the best judge of the mind of others. Downey's results pointed in the same direction. But all that the contrast shows is that the rule does not hold for the present material. As will be seen later, the process of adaptation gives to the comic situation a changing rather than a static value. As will be pointed out in the discussion of the 'chestnut' process, in the average opinion

of the group, some of the jokes change greatly in value with successive repetitions, one class rising in the relative scale, another class just as decidedly falling, while only about half of the cards approximate their original positions in the later arrangements. This means that if a given individual's judgments are to be an index of the opinion of the group, his evaluation of the waxing or waning jokes must vary correspondingly, thus giving him a high M.V. In so far as the individual's consecutive arrangements remain uniform, in just that far does he come short of being representative of his group. It is clear from this result that in all such measurements the *stability* of the material must be in some way ascertained before the results can be safely interpreted. In an unpublished study, by the writer, of the relative strength of appeals, the results of Wells and Downey are confirmed although the correlation is not high, being about +.30 with a P.E. of .08. The material for this experiment consisted of 50 descriptions of an ideal commodity, each description appealing to a different interest, instinct or effective conception. The descriptions were arranged in strict order of merit according to the persuasiveness of their appeal to the reader. Nine of the observers in the experiments on the comic took part in this experiment as well. Two sortings, one month apart, were made, and each individual's personal consistency, as measured by the correlation of her two trials, compared with her approximation to the group average, as in the present experiment. Table VIII. shows for each observer the consistency coefficient for each experiment, and also her correlation, in each, with the group average.

The correlation of the individual positions in the consistency columns

$$\left(r = 1 - \frac{6\Sigma(d^2)}{n(n^2-1)}\right)$$

is −.30. In so far as this coefficient is reliable it indicates the absence of such a thing as general consistency in the two experiments. Of course for so few cases the correlation coefficient of −.30 is not very reliable, the probable error $(.6745 \cdot 1 - r^2/\sqrt{n})$ being .20.

But there is nothing to indicate that the individual whose judgments are variable in one experiment will also be found variable in another.

TABLE VIII

Observer	Personal Consistency		Correlation with Average	
	Appeals (r)	Comic (M.V.)	Appeals	Comic
Ellison	.55	.88	.24	.32
Mahon	.13	1.65	.36	.55
Mordecai	.71	1.30	.13	.54
Denike	.78	1.86	.52	.66
Gerstein	.81	.95	.66	.70
Maschmedt	.87	1.43	.36	.60
Pratt	.74	1.35	.62	.28
Biwhop	.73	.87	.43	.30
Schlesinger	—	.87	—	.43
Hart	.8	.92	.55	.48
	$r=-.30$		$r=+.22$	

The coefficient for the correlation of approximation to the group average in the two experiments is +.22. This figure is subject to the same high probable error as is the consistency coefficient, but in so far as it is reliable it indicates a certain degree of general judicial capacity, the individual who is the best judge of her group in one case being somewhat more likely than any other to be the best judge of the same group in another case.

ADAPTATION TO THE COMIC

The writer's principal interest in carrying out the experiment lay in the study of the judgments themselves, but examination of the behavior of the cards from trial to trial discloses several clear differences. If for each trial the average value of each card be taken and the whole 39 arranged in a strict order of merit, some of the cards are found to rise in their relative position from trial to trial, others to fall as uniformly, while another large group either remain stationary or vary irregularly. This means that, aside from changing in absolute value, the jokes also change in relative merit under the influence of repetition. The group as a whole, as measured by the total value of points, we have already seen to fall in value. But some of them fall much less rapidly than others, so that cards which are originally ow in the scale

mount higher and higher in relative position, while cards originally more or less high drop lower and lower. The result is that when the curves are drawn for the five successive trials, the cards differentiate themselves into a waxing, a waning and a static group. Table I. has already shown the position of each card in each trial and the following plates show the behavior of the waxing and waning groups, the heavy line in each representing the average of the total minor group. The behavior of these cards can also be calculated by comparing their individual position in the 10 piles into which they were each time sorted, thus using their approximate absolute value instead of their relative position. When this is done the curves of Plate III. are secured, in which are shown the 'absolute' change in value of the whole group and of the three minor groups (waxing, waning and static). If we assume that the observer's absolute 'standard of funniness' remained constant from week to week the relatively waxing group will have increased in absolute value as well as in relative position within the group, and we would then have a class of jokes which not only did not suffer adaptation but actually increased in funniness with increasing familiarity.

The reader will recall that the 39 jokes were classified by 10 observers under the 10 categories of the comic. Table IX. gives the score of the votes for each of the 39 jokes. Comparison of this table with the position of each card in the waxing, waning or static groups shows that the waxing group consists entirely of naïve jokes and of calamity jokes in which the predicament of the victim is self-induced; the waning group consists of the sharp retort, the pun or play on words, wit, caricature and the occupation joke; the static group includes jokes in which the nature of the category is not clear, as evidenced by the scattering votes of the judges, of a few naïve in which the naïveté involves or resembles the play on words, and the calamity joke in which the victim's predicament is brought about by the design or retort of a second party. This result seems to tally well with the facts of introspection and of daily observation. The artless remarks of children, the reply or slip which unintentionally yields amusement,

TABLE IX

CLASSIFICATION AND BEHAVIOR

No.	Carica-ture	Fun or Play on Words	Dialect	Wit	Calamity	Nälve	Sharp Retort	Misplaced Modifier	Occupa-tion	Vulgarity, etc.	Behavior
1		7				2	1				Wane.
2	1	4		1		4					Stat.
3		2			5	2				1	Wax.
4		2			1		7				Wane.
5	1			4	1	3			1		Stat.
6	1			1		5	1		2		Wax.
7								10			Wane.
8				1		4			5		Stat.
9		1		4		1			4		Wane.
10	1	1			2	6					Wax.
11	3	1	1			3			2		Stat.
12		1		2		3	4				Stat.
13					8	1				1	Wax.
14		7	3								Stat.
15		5				5					Stat.
16				1		7			2		Wax.
17		2	1		6	1					Stat.
18		4				5			1		Wane.
19				2		8					Wax.
20							10				Wane.
21				3			7				Stat.
22		4	1	1	1	2					Stat.
23				1			8			1	Stat.
24						8	2				Stat.
25	5		1	2		1			1		Stat.
26						10					Wax.
27		2				7				1	Wax.
28	2				3	4		1			Stat.
29	1			1		8					Stat.
30				6	1		2			1	Wane.
31						10					Stat.
32						10					Wax.
33	3		2			5					Wax.
34						10					Wax.
35		9		1							Wane.
36				2	3		5				Stat.
37		8				1				1	Stat.
38	1			2	1	3	2			1	Wax.
39		10									Stat.

the harmless predicament in which the weakness, stupidity, absent-mindedness or excitement of men involve them, the Jack and Jill episodes of street and drawing room, the sudden

PLATE I

The Waxing Group

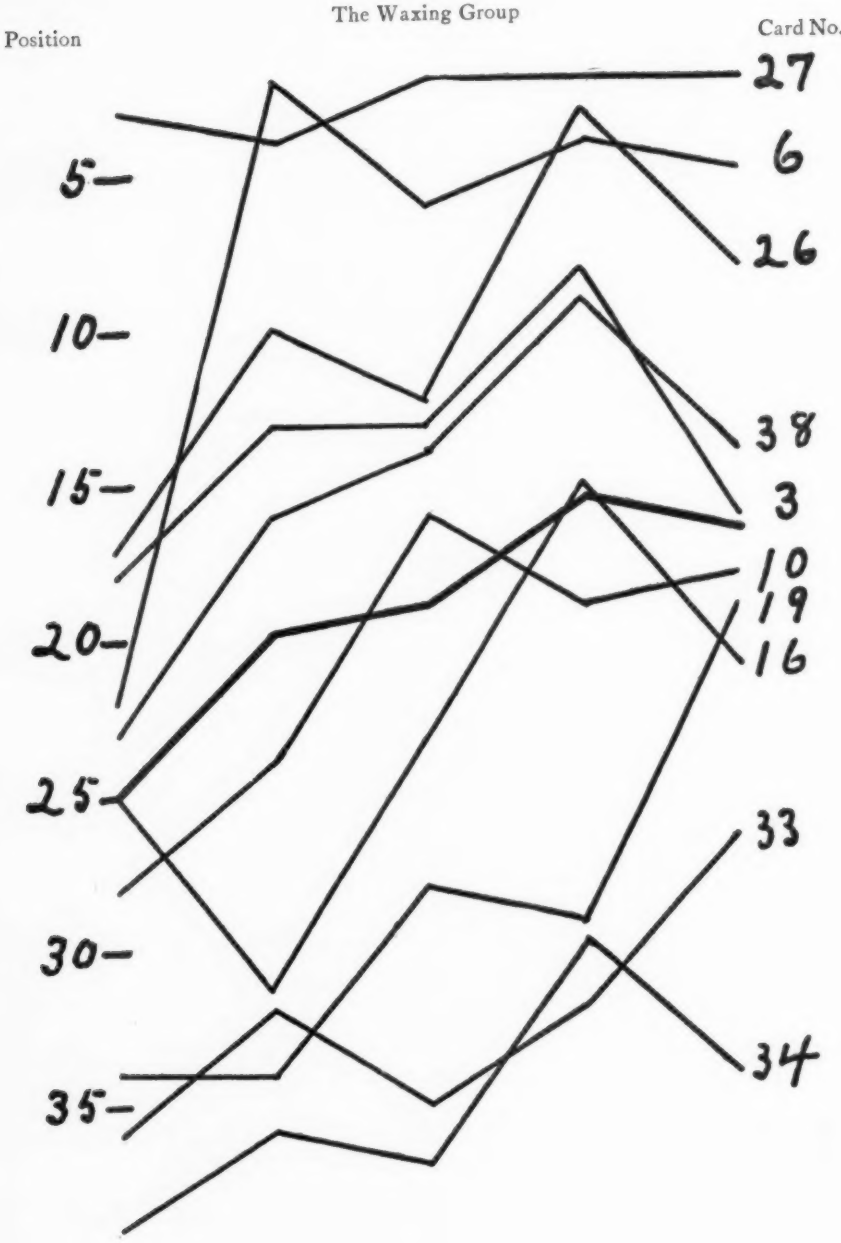


PLATE II
The Waning Group

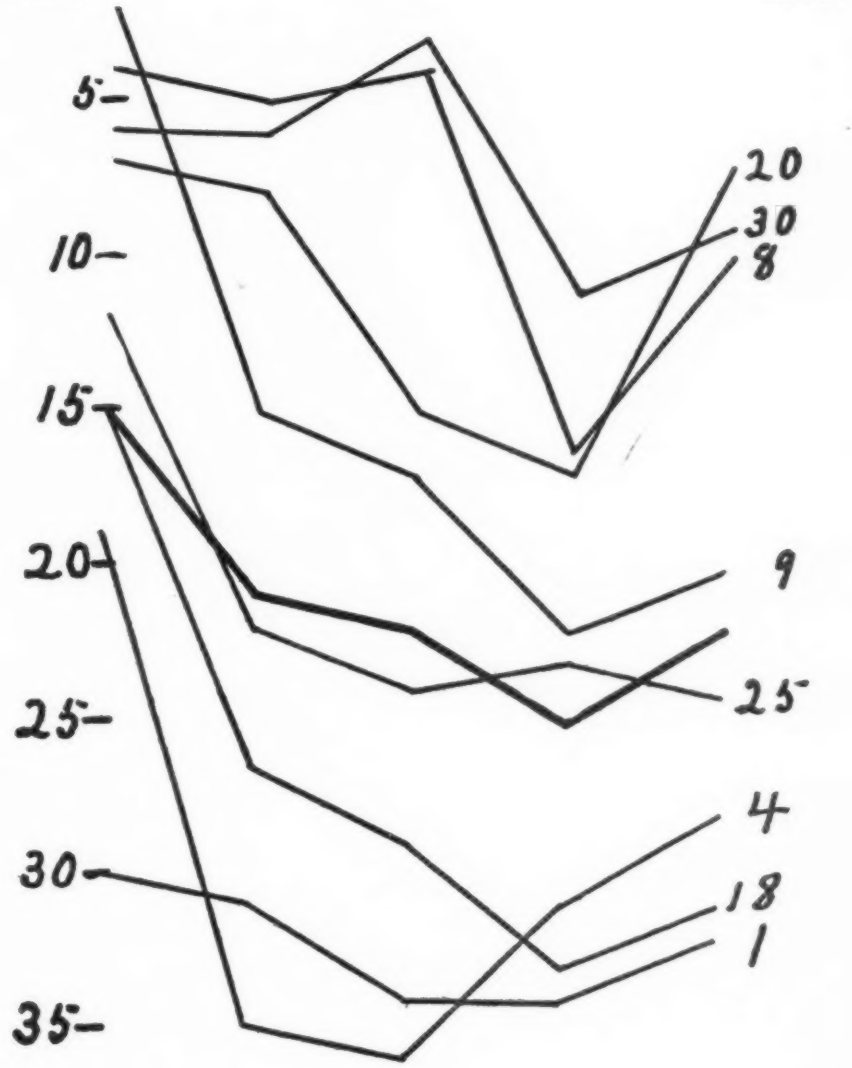
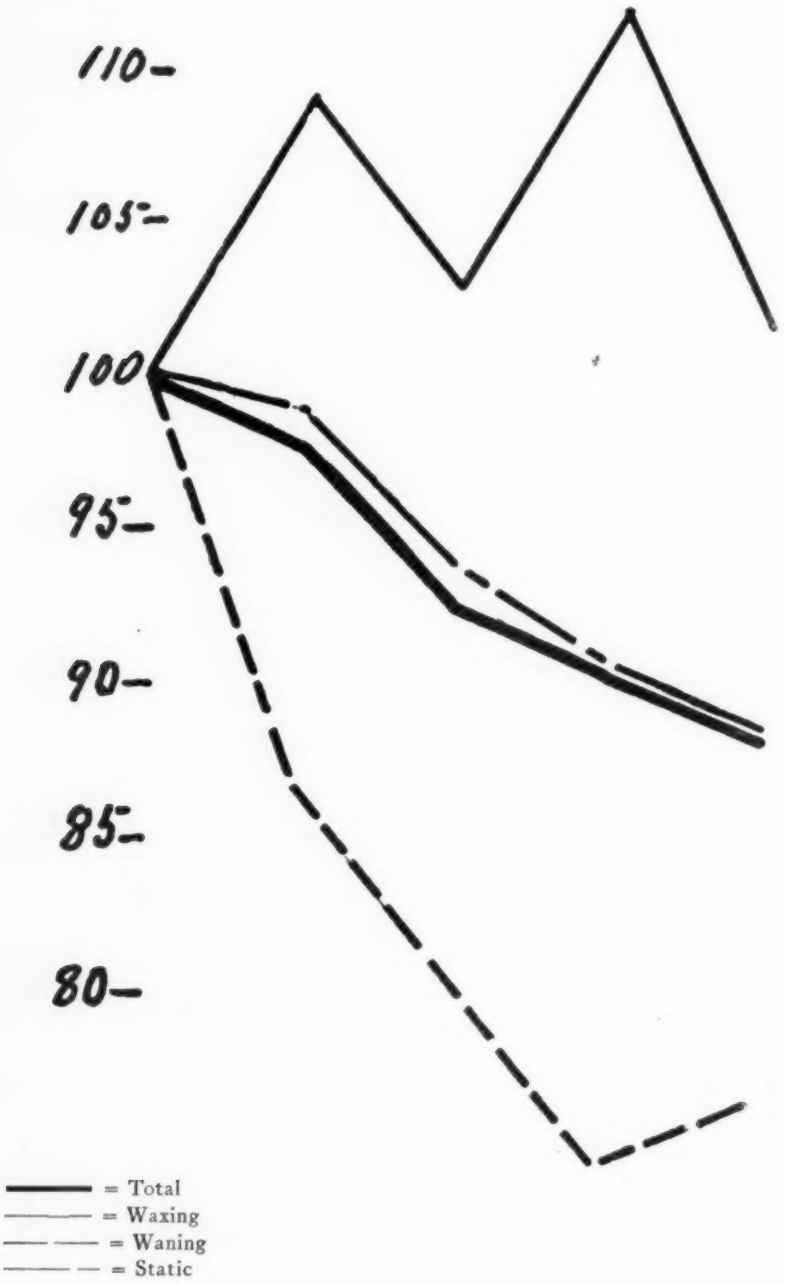


PLATE III

Change in "absolute" value in terms of per cent of original value.



revelation of unguarded human nature, etc., do not soon lose their laughter-provoking power. The whole performance is objective, does not involve any deception or subordination on our own part, and even anticipation does not rob the event of its comicality. But the pun, the witty stroke, the sharp retort, jokes whose original success depended on the degree to which they misled, deceived or tricked our own mental processes of judgment or expectation will not so well endure repetition because our subjective deception is no longer forthcoming. Anticipation and repetition cause such jests to wane rapidly, just as the present experiment indicates.

Now the static joke is likely to be one which involves both this subjective and objective relation,—thus if the calamity is self-imposed by the victim, the success of the stroke depends not so much on any deception, surprise or incongruity in the observer's mind as on the character of the spectacle itself, and the joke waxes. Whereas if the calamity is induced by the design of another it is likely to depend for its success on our own surprised reaction to a sharp retort or an unexpected verbal relation. Here then two tendencies are at work,—the calamity element which waxes, and the retort element which wanes. The net result is that the joke falls into the static minor group, neither rising nor falling in relative position, at least doing neither with regularity. Chance remarks of the observers as they went through the sortings from week to week clearly confirmed the above results, but no complete record was kept of these statements nor of the observer's introspections.

Another interesting point in the behavior of the cards is indicated in the general rise of the waning and fall of the waxing cards on the last day. This tendency to reversal seems to have been caused by the fact that, for the encouragement of the observers, who had by this time come to be somewhat bored by the performance, each was told that this was to be the last time. The effect of this remark seems to have been to increase the value of the poor jokes, so that, although the pack as a whole still dropped in value, the waning group fell more slowly than before. The final effect was that the

waning group seemed to become relatively better and the waxing relatively poorer.

The writer does not care to attempt in the present paper a thoroughgoing application of the data here reported to the theory of the comic, though such an analysis would not be at all out of place with such clear-cut results. Schauer[1] has recently attempted to show that all the forms of the comic rest on a primitive play impulse,—the delight in banter, play-fighting, 'neckerei,' and that what causes the laugh is not the 'incongruity,' 'unexpectedness' nor 'suddenly relieved tension' of the experience nor any 'feeling of superiority' in the observer, but solely the success of the trick as a play activity, the comic being here, as Miss Smith[2] has already suggested, only one form of the successful experience. Schauer then divides the comic into two types,—the objective-comic, in which the trick is perpetrated on another by natural forces, by a third person, or by the victim's own blunders, and the subjective-comic, in which we are ourselves the object of the trick and feel ourselves led on, duped or disconcerted.

The results of the present experiment accord nicely with Schauer's analysis, the waxing group corresponding to his objective-comic, and the waning group to his subjective-comic, while the static group contains both elements, either for the same or for different observers. It appears, furthermore, that the same situation, such as a stage fright, a social *faux pas* or a financial embarrassment may be originally decidedly waning in quality, but as the circumstance becomes an affair of memory and the victim is increasingly able to view his own previous predicament from the spectator's point of view, the 'oblivescence of the disagreeable' gets in its work and the experience passes from the waning into the waxing category.

[1] Schauer, O. Ueber das Wesen der Komik. *Archiv. f. d. Ges. Psychol.*, **18**, 1910, p. 410.

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[1] Information as to any omissions or inaccuracies in this list will be gratefully received if sent to Henry James, Jr., 95 Irving Street, Cambridge, Mass., or to Edwin B. Holt, Harvard University, Cambridge, Mass.

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EDITORIAL ANNOUNCEMENT

The officers of the International Congress for Psychology, 1913, consisting of the two Vice-presidents, Professors E. B. Titchener and J. McKeen Cattell, and the secretary, Professor John B. Watson, voted some time ago to ask the past presidents of the American Psychological Association to serve as Vice-presidents of the Congress. At a recent meeting in New York of the enlarged body of officers, called by Professor Cattell, details and plans for the organization of the Congress were discussed. It was decided to hold the Congress at Easter, 1913. The executive committee which now jointly represents the interests both of the Congress and of the American Psychological Association consists of the following members:

JAMES R. ANGELL
W. V. D. BINGHAM
J. MCKEEN CATTELL
HUGO MÜNSTERBERG
E. C. SANFORD
E. B. TITCHENER
JOHN B. WATSON